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Second Edition

Comparative Politics

RATIONALITY,
CULTURE, AND
STRUCTURE

Edited by

MARK IRVING LICHBACH

ALAN S. ZUCKERMAN

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Comparative Politics

Second Edition

Comparative Politics: Rationality, Culture, and Structure is the completely revised second edition of the volume that guided thousands of scholars through the intellectual demands and gratifications of comparative political science. Retaining a focus on the field's research schools, it now pays parallel attention to the pragmatics of causal research. Mark Irving Lichbach begins with a review of discovery, explanation, and evidence, and Alan S. Zuckerman argues for explanations with social mechanisms. Ira Katznelson, writing on structuralist analyses, Margaret Levi on rational choice theory, and Marc Howard Ross on culturalist analyses, assess developments in the field's research schools. Subsequent chapters explore the relationship among the paradigms and current research: Joel S. Migdal examines the state; Mark Blyth adds culturalist themes to work on political economy; Etel Solingen locates the international context of comparative politics; Doug McAdam, Sidney Tarrow, and Charles Tilly address contentious politics; Robert Huckfeldt explores multilevel analyses; Christopher J. Anderson describes nested voters; Jonathan Rodden examines endogenous institutions; Isabela Mares studies welfare states, and Kanchan Chandra proposes a causal account of ethnic politics. The volume offers a rigorous and exciting assessment of the past decade of scholarship in comparative politics.

Mark Irving Lichbach is Professor and Chair of Government and Politics at the University of Maryland. A theorist interested in social choice and a comparativist interested in globalization, Lichbach explores the connections between collective action theories and political conflict as well as the connections between collective choice theories and democratic institutions. He is the author or editor of many books, including the award-winning *The Rebel's Dilemma*, and of numerous articles that have appeared in scholarly journals in political science, economics, and sociology.

Alan S. Zuckerman is Professor of Political Science at Brown University. His most recent books are *Partisan Families: The Social Logic of Bounded Partisanship in Germany and Britain* (with Josip Dasović and Jennifer Fitzgerald), the winner of the International Society of Political Psychology's award for the best book published in 2007, and *The Social Logic of Politics: Personal Networks as Contexts for Political Behavior* (2005). Zuckerman edits the book series on *The Social Logic of Politics* for Temple University Press. During the spring semester 2007, he was the Lady Davis Visiting Professor of Political Science at the Hebrew University of Jerusalem.

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Comparative Politics

RATIONALITY, CULTURE,
AND STRUCTURE

SECOND EDITION

Edited by

MARK IRVING LICHBACH

University of Maryland

ALAN S. ZUCKERMAN

Brown University



CAMBRIDGE
UNIVERSITY PRESS

CAMBRIDGE UNIVERSITY PRESS

Cambridge, New York, Melbourne, Madrid, Cape Town, Singapore, São Paulo

Cambridge University Press

The Edinburgh Building, Cambridge CB2 8RU, UK

Published in the United States of America by Cambridge University Press, New York

www.cambridge.org

Information on this title: www.cambridge.org/9780521885157

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First published in print format 2009

ISBN-13 978-0-511-47935-9 eBook (EBL)

ISBN-13 978-0-521-88515-7 hardback

ISBN-13 978-0-521-71234-7 paperback

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*For
Faye
and Ricki,
again and always*

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Preface and Acknowledgments

Who now reads Max Weber? Seventy years ago, Talcott Parsons began his magisterial study of social thought by asking Crane Brinton's question about Herbert Spencer. Parsons demonstrated that by the end of the first third of the twentieth century, Weber's, Durkheim's, Pareto's, and Marshall's scholarship had eclipsed Spencer's social science and of the four theorists, Weber deserved pride of place. Has knowledge of Weber's social science among the current generation of students and professors of comparative politics mimicked the earlier abandonment of Spencer's social analysis?

When the senior comparativists in this volume attended graduate school, they studied Max Weber. From him, they learned about theories of regime types and transformation, economic development, bureaucracies, rationality, religion and politics, philosophy of social science, the tension between formal government and democratic processes, and a deep critique of modernity and of Marxist theory, among a host of basic matters. Most important, they developed an appreciation for grand questions of politics encased in theory and method.

Today, graduate studies in comparative politics more closely resemble professional training than library reading. Highlighting methods, students are taught that the best work combines fieldwork, interviews, surveys, archival work, experiments, and statistics, as well as formal models. Explaining substantive puzzles with causal arguments buttressed by extensive methods occupies the field's center. Successful scholarship displays the best techniques of analysis.

Weber understood that there is no opposition between mastering methods and social, economic, and political analysis. Moreover, he taught that scholarship demands examination of important questions, offering theoretically informed causal stories as well as applying the most appropriate methods. And so, following Weber, we have organized this volume around the interaction between paradigms – approaches to ontology, theory, and methods – and the pragmatics of causal analysis.

In this second edition of our volume, our colleagues and we interpret the state of comparative politics today, more than a decade after the first volume. The field has changed, and so the essays analyzing it have changed too.

Another reason to offer a second edition is to update the service provided to young scholars of comparative politics across the globe. The volume serves as a survey and analysis of the field, and apparently, cohorts of graduate students have read and benefited from the essays in the first edition. We want to thank them for being attentive readers and to thank their teachers who thought that the volume would benefit their students. We hope that the second edition plays the same helpful role in the lives of many other graduate students in political science.

One of the gifts of publishing is the opportunity to express public and formal appreciation to people who offer assistance. It is a pleasure to thank Lew Bateman, our editor at Cambridge University Press, a persistent and wise guide, and our colleague Margaret Levi, who also leads (as well as edits) the Press's series on comparative politics, in which the first and this edition of the book appear. It is an honor to be in such impressive company. Thanks too to the Rockefeller Foundation for sponsoring a meeting at the Bellagio Conference and Study Center in October 2006. Conversations and exchanges of views at their superlative site initiated the work of this second edition. In addition, we offer our gratitude to several authors of the chapters – Margaret Levi, Ira Katznelson, Marc Ross, and Sidney Tarrow – for wise counsel as the project has unfolded.

Paradigms and Pragmatism

Comparative Politics during the Past Decade

Mark Irving Lichbach and Alan S. Zuckerman

INTRODUCTION

Students of comparative politics explain electoral behavior, political networks, political institutions, contentious politics, comparative political economies, welfare states, international-comparative linkages, and the state. Their interest in the pragmatic and causal analysis of these critical political questions defines the “messy center”¹ of comparative politics. The first edition of this volume emphasized the field’s research paradigms, placing rationality, culture, and structure in the subtitle. In the decade or so since the first edition was published, tension between these two perspectives on the field has persisted. Contrasts between research paradigms and pragmatic causal accounts provide the intellectual friction that drives much of our research. These alternative foci structure this edition’s themes and problems.

Aiming to transcend a battle of the paradigms, Alan Zuckerman’s chapter advances an explanatory strategy that is one such way forward. Explanations in comparative politics, he maintains, must meet clear standards: The more that are met, the better the results. The criteria include social mechanisms (a particular form of causal mechanism) that are derived from strong theoretical propositions. Convincing explanations also require empirical evidence of the specified explanatory processes. Because the ontology of politics demands that the explanations apply to stochastic, multilevel, and endogenous phenomena, simple causal claims are insufficient.

Applying social mechanisms with high prior probabilities of explanatory power and employing appropriate statistical techniques transforms the language of explanation from imprecise verbal accounts into clear and specific arguments. The results move explanation along a scale from the mistaken to the demonstrated. As an attempt to convince by doing, Zuckerman applies his message

¹ This characterization of the field appeared first in the symposium in *World Politics*, on the role of theory in comparative politics, published in 1996. It is meant to convey a multimethod approach that draws from many theories.

Our thanks to Sidney Tarrow and Joel Migdal for comments on earlier drafts of this chapter.

to research on partisanship and on political violence. In his view, research paradigms are a source of strong explanatory hypotheses, but they are not the sum total of scholarship in comparative politics.

Mark Lichbach's chapter advances a somewhat different perspective on contemporary comparative politics: While overt paradigm wars have been dampened, paradigm-driven teaching and thinking persists. In the field's tool-boxes and cookbooks, paradigms continue to provide the content and direction – the underlying purpose and logic – for many contemporary comparativists. Most importantly, they fuel the field's creative impetus.

The second edition of *Comparative Politics* assesses the role that research paradigms and pragmatic explanatory strategies currently play in the field. In order to bring assessments of the debate among the paradigms up-to-date, we asked our authors to address several questions:

How have the dynamics among rationality-culture-structure played out?

What are the different types of responses to the battle of the paradigms?

How do scholars currently treat the approaches? Are rationality-culture-structure comparisons no longer central to the field? Do researchers still begin their research with an interparadigmatic dialogue in mind? Do they still use the debate to evaluate existing theories? When developing new theories, do they still return to the debate?

Have the paradigms converged or do they remain distinct? Which metaphor best characterizes the field: separate tables, a messy center of convergence, or a mixed bag of partial synergisms? Is there a new paradigm war, with culturalists and constructivists as today's paradigm warriors, on the horizon?

Do multiple perspectives shed more light than heat? Are creative research moves often based on appeals to ideal-type paradigms? Does competition among paradigms move the field and promote progress by generating critical reflection, fashioning significant evidence, and improving important concepts?

While the authors of our theoretical chapters – on rationality, culture, and structure – and of our substantive chapters organize their contributions in their own ways, all examine the field's paradigms and pragmatic strategies of explanation.

THE CHAPTERS

We begin with two general chapters, Lichbach's assessment of efforts to move past the debate about research schools and Zuckerman's attempt to improve explanations in comparative politics. Both highlight the volume's links between paradigms and causal analyses. Structural and rationalist analyses applied to the messy center follow. Ira Katznelson and Margaret Levi provide theoretical overviews of structure and rationality, respectively, that demonstrate an

emerging consortium. We then turn to movements against the mainstream. Marc Ross offers a theoretical overview of culture. Joel Migdal's discussion of the state indirectly points to, and Mark Blyth's analysis of comparative political economy more directly discusses, the growing significance of constructivism. Two chapters then illustrate the center's dialogue between research paradigms and causal pragmatism: Etel Solingen considers global-domestic linkages and Doug McAdam, Sidney Tarrow, and Charles Tilly analyze the field of contentious politics. The volume then considers ways that comparativists fortify the field's center by elaborating causal explanations. Robert Huckfeldt's analysis of political networks and Christopher J. Anderson's review of the literature on mass political behavior emphasize macro-micro connections and the need for multilevel analyses. Uncovering endogenous causal relationships provides the theme of Jonathan Rodden's review of institutions and political economy. Isabela Mares delves deeply into causal analyses of welfare states. Finally, Kanchan Chandra discusses the problem of making causal claims about ethnicity and politics, the research domain in comparative politics that has been most affected by recent constructivist thought.

The Messy Center: Big-Picture Pragmatism

"Now we political economists have a pedantic custom," writes Max Weber in his classic essay "Science as a Vocation" (1946: 129), "which I should like to follow, of always beginning with the external conditions." Following Weber, Ira Katznelson would have comparativists begin with the contemporary world's major force – modern liberalism, as it developed in the West. Katznelson suggests that robust problem-solving scholarship and the revival of large-scale studies have renewed the prospects of institutional studies that combine strong theory, a configurational approach to causality, and respect for history's variety. These efforts continue established research programs on the dominant structures of modernity – capitalism, civil society, the state, and the state system. A close critical focus on liberalism offers comparative politics a similarly energetic focus.

Even as Katznelson sees the biggest of pictures, he adopts an eclectic research strategy. In a theme that reappears in many of the chapters, Katznelson demonstrates a "pragmatic attitude about method." Displaying a "healthy disrespect for overly stylized battles about paradigms," he wants to employ "a range of analytical traditions to answer tough and meaningful questions" about "important problems." Katznelson urges "problem-focused writing that exhibits little respect for traditional divisions within comparative politics" such as political economy, contentious politics, and electoral studies. Utilizing multiple methods – archives, surveys, ethnography, experiments, and cross-national statistics – helps the field transcend "inductive variable-centered strategies." He also urges comparativists to "refuse to choose between positive and normative orientations." Believing that many "intersecting modes of investigation" can produce findings that illuminate questions that are empirically grounded, ones

rich in knowledge of time and place, Katznelson advocates a style of comparative inquiry that is “realist and concrete rather than nominal and abstract, [one] aimed at discerning a ‘sweet spot’ located in the zone between high abstraction and particular specification.”

Even so, Katznelson cautions against excessive pragmatism. Following ideas he advanced in his chapter for the volume’s first edition, Katznelson has little use for “highly targeted studies” of limited ambition that produce “substantive and conceptual retrenchment” from the great works of the past. Without the sort of larger project focused on Western liberalism that he advocates, “thematic literatures threaten to remain confined within specialized conversations, and possibilities for integrating findings across a range of discoveries are likely to stay artificially abridged.” Katznelson thus worries about the decentering of comparative politics – the heterogeneity and diversity in subjects, questions, and studies that inevitably accompany a diverse toolkit. Katznelson seeks a big-picture pragmatism that can contain the field’s tensions and contradictions.

Applying a rational choice approach, Margaret Levi also advocates research pragmatism that aims at big questions. Her chapter details significant substantive, methodological, and theoretical advances in rational choice analysis that allows rationalists to employ manageable research strategies to probe the big picture. Levi discusses how, over the past decade, rational choice comparativists have indeed helped to redirect comparative politics toward goals that she shares with Katznelson.

The successes of the comparative and historical mode of rational choice theory derive in part from debates with culturalists and structuralists. Even as convergence across the research schools grows, Levi maintains, paradigms remain: “While the paradigm wars . . . have certainly subsided, they have not disappeared entirely. Paradigmatic distinctions remain relevant both to training and to research.” She further notes, “what divides [paradigms] is method in the sense of how to construct theory and organize research findings. Rationalists continue to emphasize methodological individualism and strategic interaction.” While some debates remain, the best comparative work, Levi claims, now uses many sophisticated methods, involving some mix of field work, interviews, surveys, archival work, experiments, and statistics in addition to formal logic. She thus advocates a “multiplicity of methods as well as approaches” that “blurs the lines among approaches” and is “methodologically pluralistic.” As Levi puts it, “not everyone does everything, but everyone seems to do several things.”

By urging comparativists to “combine a nuanced understanding of the complexity of a particular (often unique) situation or set of events with a general theoretical understanding,” Levi echoes Katznelson’s big-picture pragmatism. Rational choice theory ensures that research has microfoundations, paying attention to the constraints on and the strategic interactions among the actors whose aggregated choices produce significant outcomes. A comparative and historical sensibility ensures that research respects context, which means that comparativists address important empirical and normative concerns. From their

different starting points, Katznelson and Levi place historical and rational choice institutionalism at the very attractive messy center of comparative politics.

Pushing against the Mainstream: Culture and Constructivism

Marc Ross is less willing to accept the field's current configuration. He reminds comparativists how culture is important to the study of politics: It provides a framework for organizing people's daily worlds – locating the self and others in them and making sense of the actions and motives of others – for grounding an analysis of interests, for linking identities to political action, and for predisposing people and groups toward specific actions and away from others. Moreover, “placing the concept of culture at the center of analysis,” Ross maintains, “affects the questions asked about political life.” Culture organizes meanings and meaning-making, defining social and political identity, structuring collective actions, and imposing a normative order on politics and social life.

Taking culture seriously means moving toward “a strong view of culture,” one that entails an “intersubjective understanding of culture.” Ross believes that “reducing culture to the sum of individual attitudes,” as is found in survey research, “is hardly adequate . . . culture is not a property of single individuals. Rather, it is an emergent property rooted in social practices and shared understandings that cannot be uncovered through survey data alone.” Even though many comparativists may be unfamiliar with the “interactive, constructed nature of culture,” he believes that this approach can make a significant contribution to the study of comparative politics.

Ross's research pragmatism draws him close to Katznelson and Levi at the field's center, as it distances him from culturalists whose postmodern relativism stresses the highly constructed nature of reality. Like all the authors in this volume, he agrees that “comparison is central to the social science enterprise” and that it employs many different sorts of evidence: “The most successful work linking culture and politics will not rely on only one source of data or a single tool for data analysis.” Applying a full range of evidence in the pursuit of causal analyses draws Ross's approach toward the field's messy center.

Joel Migdal places the “comparative politics of the state” at the field's center, even as he also respectfully moves apart from the mainstream. Migdal suggests that comparisons have relied heavily on a universal template or image of what the state is and does. This universal standard has strained under the widening diversity of states, especially those formed after World War II. Appreciating the effects of globalization, his chapter offers an alternative understanding of this critical concept.

Studying the state, Migdal suggests, involves probing a multilayered, multi-purpose entity whose parts frequently work at cross-purposes. This political institution operates in a similarly complex multitiered environment, which deeply affects the state and, in turn, is affected by the state. “All this complexity has turned the experience of researching the state into an eclectic enterprise. It demands a full toolkit – an amalgamation of culturalist, structuralist, and

rationalist tools and of historical, case, and quantitative methods” because “different perspectives highlight the variegated visages of the state and their interactions with their domestic, regional, and global environments.” Appropriate research combines quantitative large-N research and qualitative single-case analysis and new forms of historical analysis, charting new directions in comparative research. Good research contextualizes the state, seeking to “combine specialized country or area knowledge (which usually is focused on the different practices of diverse states) with more general theories of state formation and behavior.” Furthermore, research is most valuable when it moves from linear, causal models toward process-oriented analysis and from comparative statics to historical analysis, emphasizing the importance of temporality and of sequencing.

How does Migdal relate to the field’s paradigms? Consistent with his explanatory pragmatism, he recognizes the importance of rational choice analyses of the state: “States’ political trajectories [are] deeply influenced by the give-and-take, negotiation, collaboration, and contestation between central state authorities (themselves sometimes fragmented) and dispersed, but locally powerful, social forces.” However, Migdal expresses reservations about rationalist theorizing: “The population is not simply an aggregate of diverse rational individuals but a collective that transcends those individuals and that gives birth to, and then loyally engages and stands behind, the state.” While recognizing the “actual baffling diversity of states,” Migdal is not interested in the rationalist program of exploring how institutions aggregate this variation into state policies and practices. His references to the “transcendental unity of the people,” to “transcending aggregated individual preferences,” and to the “general will, legitimacy, social solidarity, and unity of allegiance” are likely to make rationalists uneasy. Interested in the convergence of history and institutions, he maintains that contemporary approaches are “neglecting culturalist factors” and that “the cultural approach still seems generally to get short shrift.” Migdal’s stress on the significance of culture as understood within institutions uncovers the fragile unity of the messy center of comparative politics.

As Mark Blyth attempts to define the field of political economy, he offers another respectful critique of the mainstream. His chapter begins by noting that “hard-won empirical research showed that the economy was inseparable from politics. Modern political economy showed that if one wanted to understand significant variations in economic outcomes, then embracing the mutual implications of states and markets was a pretty good place to start.” One does not do political economy, according to Blyth, by beginning with the research paradigms of rationality, culture, and structure, as if they contain toolboxes of foundational heuristics. Rather, he suggests that political economists employ a “troika of ‘interests,’ ‘institutions,’ and ‘ideas’” in which “all three of these positions are vibrant research programs.”

Like Ross, Blyth advances constructivist claims about the importance of ideas. He contends that “exogenous shocks to agents’ material positions do not unproblematically translate into new political preferences” because “exogenous

economic changes rarely, if ever, telegraph into agents' heads 'what has gone wrong' and 'what should be done.'" Many political economists eventually recognized that "ideas and ideologies needed to be taken seriously as explanatory concepts in their own right." Put differently, "ideas do not merely describe the world; they also help bring that world into being." The "particular construction[s] of the political economy agents develop and deploy help bring into being that which is described rather than simply describing an already existing state of affairs." Comparativists thus should be "investigating how the action of employing ideas that seek to represent or measure a given phenomenon brings the phenomenon into being." "What globalization 'is,'" for example, "is itself constructed differentially across nations" by different sets of actors.

If "agents' subjectivities and interests can be reconstructed despite their ostensible structural positions," constructivists wonder whether materialist theories of history reinterpret history as per their theories, "sacrificing historical accuracy for theoretical fit." Do actual political actors ever think the way that the theories say they do? In other words, "can one really link actors' intentions to outcomes via their material interests, as this literature presumes"? As "ideational approaches drop below the level of the possible to investigate what real actors thought and did," they challenge mainstream thinking about interests and institutions. "Once 'let out of the box,' ideas 'have a life of their own' and can take interests in new and unexpected directions." Blyth thus warns, "if contingency, construction, and interdependent effects are as replete as at least some of these [constructivist] scholars say they are, then the question of whether political economy can aspire to the status of a predictive science is questionable at best."

During the past 10–12 years, these chapters suggest, comparativists have responded to the field's research paradigms in alternative ways. While Katznelson and Levi depict a convergence around the study of history and institutions, Migdal introduces and Ross and Blyth deepen a culturalist critique of this perspective. Katznelson and Migdal offer big concepts: liberalism and the state, respectively, as unifying themes for future scholarship. All, however, share a vision of research that pragmatically draws on an eclectic array of tools.

Fortifying the Center: Research Paradigms and Causal Analysis

Given the disparate research tools in comparative politics, comparativists confront thorny questions about research schools and causal explanations. As the volume proceeds, the chapters move more deeply into these issues. The next two chapters, Etel Solingen's analysis of global-domestic linkages and McAdam, Tarrow, and Tilly's exploration of contentious politics, highlight the efforts to speak both to paradigms and to pragmatic demands of causal research.

The relationship between comparative and international politics has a long intellectual pedigree – indeed, the distinction between the two fields confuses laypeople. Etel Solingen brings the literature on the international sources of domestic politics into the era of globalization. Focusing primarily on work published since the volume's first edition, she examines comparative politics as

it has become increasingly global in scope and interest. Her chapter relates both international influences on domestic politics (Type A effects) and domestic influences on international politics (Type B effects) to representative work in the structural, rational, and cultural traditions. States, political parties, social movements, peak associations, labor unions, policy networks, armed forces, and other collective and individual actors respond to global opportunities and constraints in various ways, suggesting far more contingency than determinacy.

As the field advances, Solingen argues, comparative and international politics draw together. Relatively simplistic understandings have given way to more nuanced and sophisticated explanations of Type A and B effects, backed by various forms of evidence. Neither purely structural nor methodological-individualist reductionisms have become modal forms of analysis, as “hybridism and mutually profitable intellectual exchange” have become dominant. Studies avoid procrustean temptations to reduce politics to rigid paradigms. As complexity deposes Occam razor’s (*lex parsimoniae*) as a standard for studies of globalization, the advantages of theoretical frugality in pure paradigmatic research seem to be progressively exchanged for the virtues of completeness and empirical validity.

Lauding “the conceptual and methodological diversity [that] comparative politics has exhibited in the past decade,” as well as the “greater creativity [that] accrues from working at the interstices of different disciplines or subfields,” Solingen echoes the field’s pragmatism. Yet, continued debates over concepts and findings suggest that comparativists remain uncertain about the immediate and long-term effects of globalization. The consequence is that “studies holding on to paradigms as foundational heuristic devices have far from disappeared.” Solingen thus reminds the reader that “asking big questions forces us to distill broad important features and rely on ideal types or heuristic devices that transcended historical or ‘true’ realities.”

While analytical debates might persist, Solingen argues that the way forward employs explanatory strategies aimed at discerning causality. Siding with Katznelson, Levi, and Migdal, her proposed methodology involves “contextualized comparisons of different cases.” As an illustration of this approach, Solingen traces the divergent paths of development of Middle Eastern and East Asian countries back to their origins in domestic coalitional grand strategies. Her pragmatic use of causal explanatory strategies unites comparativists of different theoretical and methodological stripes.

Doug McAdam, Sidney Tarrow, and Charles Tilly, who are affectionately known as McTT, offer more than a review of recent scholarship on contentious politics. Like Migdal and Blyth, they define a domain of inquiry. Nonroutine, or contentious, politics is a thriving but fragmented interdisciplinary field of study, divided across a confusing patchwork of disciplinary boundaries, geographic areas, historical periods, and nominally different types of contention. Changes in the “real world” of contentious politics have forced scholars to broaden their attention from social movements in Europe and the United States to newer, more wide-ranging, and more violent forms of conflict; to contention against

nonstate targets; and to transnational contention. Their chapter traces some of these changes and puts forward a sketch of an integrated approach to a field that the authors admit is “more imagined than real.”

Research paradigms once drove the study of contentious politics. The roots of contentious politics are found in “a structurally rooted political process model,” “a rational choice perspective and its related resource mobilization variant,” and “a constructivist approach that draws, first, on an older ‘collective behavior’ tradition and, second, on the more general cultural turn in the social sciences.” Similar to what happened in comparative politics writ large, paradigm warfare led to attempts at synthesis. Navigating between Theda Skocpol’s material structuralism and Mark Lichbach’s rationalist micro-foundations, McTT’s synthesis is highly relational, dynamic, and process oriented. They prefer to study episodes rather than events and mechanisms rather than variables. Moreover, the field of contentious politics is at the forefront of the “culturalist turn” in the social sciences and comparative politics. What goes on in the contentious politics literature foreshadows the future of comparative politics.

The chapter also contributes directly to our appreciation of different patterns of explanation in comparative politics. Eschewing “general covering laws of the type ‘all collective action is aimed at producing improvements in individuals’ material situations,” McTT offer mechanisms as a way of unpeeling larger political processes: “Mechanisms are the causal links between independent and dependent variables, which we define as events that produce the same immediate effects over a wide range of circumstances.” The mechanisms of brokerage, identity shift, co-optation, diffusion, and repression relate to larger processes of political mobilization and demobilization. Assembling these mechanisms and processes, McTT argue, is the way to create the field called contentious politics.

They also suggest that “contentious politics is a causally coherent domain with distinctive properties. It is causally coherent in the sense that similar cause–effect relationships apply throughout the domain.” Though they hope that “a more thoroughgoing search for cause–effect relations spanning multiple forms of contention will be fruitful,” they believe that the successes to date have been meager. Nevertheless, viewing McTT’s research program in causal terms suggests that students of contentious politics should adopt a three-part strategy. First, “construct analytic narratives of episodes of contention.” Second, “break them down into the mechanisms and processes that drive them.” Finally, “connect them to their origins and outcomes” in particular contexts.” This last step, “delineating types of regimes and their combination of capacities and degrees of democracy . . . analyzing the interactions between regimes and forms of contention would take us far toward the construction of a comparative political science of contentious politics.”

The members of this research team think of themselves as “pragmatists” who are “catholic in our methodological judgments.” Pragmatists explore empirical causes and consequences, not transcendental foundations and

essences, and so McTT join the mainstream of comparative politics even as this research program moves beyond the boundaries of political science.

Fortifying the Center: Linking Structure and Action and Exploring Causal Patterns

The next set of chapters emphasizes the relative importance of causal analysis over research paradigms. As Solingen's chapter indicates, causal accounts in comparative politics cross levels. Huckfeldt's chapter on political networks and Anderson's review of the literature on electoral behavior highlight the linkages between context and individual choice. The second pair of chapters raises the puzzle of how endogenous political institutions become causal forces. Rodden confronts the problem of endogeneity in causal claims about institutions, and Mares show how causal accounts lie at the heart of research on welfare state institutions and policies.

Robert Huckfeldt addresses issues in democratic politics that are part of Katznelson's research agenda on modern liberalism. Suggesting that the role of the purposefully engaged citizen is central to the vitality of democratic politics, he denies that citizens are atomized individuals. To the contrary, patterns of political communication produce networks of political interdependence – people whose preferences, choices, and levels of political involvement are jointly determined by one another. Lying beyond the proximate reach of individual citizens, these patterns of communication are contingent on the distribution of beliefs and habits, as well as on the institutions and structures specific to concrete social life and particular political systems. As a consequence, both the exercise of individual citizenship and the performance of aggregate electorates are subject to institutional, cultural, and structural variations that produce important continuities, as well as discontinuities, in democratic politics.

This big problem of democratic politics is based on a simple but fundamental principle: "Politics is *not* reducible to the sum of its parts." In order to demonstrate the complexities of social networks and political communication, Huckfeldt highlights perhaps the first stylized statistical fact adduced in comparative politics: "When Tingsten plots the socialist proportion of the vote in Stockholm precincts on the working-class proportion of the precinct population, the resulting scatter follows a pronounced nonlinear s-curve pattern. At low working-class densities, the socialist parties' share of the vote falls below the working-class proportion of the population, but the vote share exceeds the working-class proportion at high work class densities. The nearly inescapable conclusion that arises on the basis of Tingsten's analysis is that the probability that individual workers (and perhaps individual nonworkers) supported the socialists varied as a function of the population composition."

Joining structure and agency in multilevel models allows Huckfeldt to study interdependent citizens. In contrast to Ross and Migdal, he constructs social structures out of individual choices and behavior. Huckfeldt thus suggests that "patterns of interdependence produce consequences for levels of

analysis problems that lie near the core of comparative political inquiry.” However, the approach is not indifferent to research schools. By tackling issues of causality, multilevel statistical models can be coupled to many of the ways of addressing the structure–action problem advocated by culturalists and constructivists.

“The comparative study of mass politics,” writes Christopher J. Anderson, “is in the process of becoming the study of nested citizens.” Stressing the direct and indirect effects of structures on behavior identified by Huckfeldt, Anderson also proposes multilevel models. He suggests that comparative mass politics should study “variations within populations rather than across countries.”

In this view, contexts are contingent effects and “a country’s democratic design matters for how voters behave.” As an example of how democratic structures influence behavior via incentives, Anderson suggests a problem at the heart of Katznelson’s liberalism: “The nature of a country’s representative structures interacts with voters’ willingness to punish governments for bad economic performance to produce different election outcomes.” While voters seek to affix credit and blame for the economy, this task is easier in countries with clear levels of responsibility. It is also more feasible where there exist credible alternatives to voting incumbents out of office. An interesting paradox results: “For high-information voters, participation in elections rises as the number of parties in the system increases. Thus, among these voters, more choices improve participation rates. However, for citizens with more limited political information, increases in the number of parties in party systems depresses voting turnout. This leads to the ironic conclusion that [proportional representation], which is normally intended to lead to a more fair representation system, increases the information gap by complicated political choices and thus disenfranchises the less informed relative to the better informed.”

More generally, Anderson explores the intellectual foundations and evolution of the behavioral study of politics. Our methods for studying comparative mass politics have been transformed, he argues, because of changes in technology, new intellectual trends, and emerging real-world events. As a result, the study of mass politics has become more central to the study of comparative politics, a pattern reinforced by its ecumenical relations with different theoretical traditions in the social sciences. Anderson thus hopes to “integrate behavioral politics within institutional politics across a wider range of theoretical concerns in comparative politics.” He suggests that “the comparative study of mass politics is in the position of playing an important bridging role across theoretical and substantive islands.”

While multilevel models can make an important contribution, Anderson recognizes that the models treat contexts as variables: “Behavioral politics, as currently practiced, is weak on process and mechanisms, but it explicitly recognizes that contexts matter.” In order to meet Zuckerman’s explanatory criteria, research on context must overcome this trade-off and study context as both causal processes and causal variables. As Anderson pragmatically

suggests, “the key question is whether a different theoretical [and, one assumes, methodological] perspective produces a compelling alternative story that can be validated with solid data.” His chapter suggests that providing causal accounts of the electoral behavior of “nested citizens” permits analysts to draw on explanatory propositions from different research schools.

Jonathan Rodden’s chapter challenges the literature that relates political institutions to policy and regime outcomes. Studies have located robust correlations between outcomes and institutions: welfare states and proportional representation, the number of political parties and electoral rules, economic development and democratic government, and lower welfare expenditures and federalism. If institutions are causes, what causes the causes? In statistical language, the endogeneity of causes leads researchers to supplement the study of the causes of effects with the study of the effects of causes. If institutions are endogenous and if “history assigns countries to institutional categories,” there might be an unobserved historical process – omitted variables – that generates both institutions and outcomes, selecting and assigning cases to both structures and policies. This means that institutions and the error term might be correlated. Operating with the equation, $\text{outcomes} = f(\text{institutions} + \text{error term})$, is then suspect. Rodden understands that technical solutions in the form of instrumental variables are inadequate quick fixes for this problem. “Rather, in order to come up with believable exclusion restrictions, one must essentially become an analytical historian. This requires more than a passing glance at the secondary literature.” Progress requires analytical histories of particular processes.

Researchers also need to employ theory, because all causal research requires assumptions. If different analytical histories lead to different choices of instruments, however, how does one know which theory to use? Comparativists try out different ones, looking for an instrument that drives the institution but not the outcome (and hence the error term). Rodden demonstrates how this is an art and not a science. For example, in order to discern the causal impact of proportional representation on the welfare state, he turns to many candidate causes of proportion representation – partisan fragmentation, a multi-round electoral format, a “strong and organized leftist workers’ movement with organizational support from labor unions,” initial income distribution, the structure of the late-nineteenth-century economy, particularly the local economic coordination produced by guilds – but without much luck. Rodden suggests that these arguments are plagued by weak microfoundations. When studies assume the presence of fully informed actors who can easily solve problems of collective action, they exude functionalist inevitability. In spite of these difficulties, Rodden does not want to “give up on the whole enterprise” and move on. He admits, however, that there are now more methods than results.

As Rodden unpacks the relationship between statistical analyses of political institutions and fundamental theoretical problems, he draws on classical structuralist concerns. While slicing and dicing reality into variables led this literature to uncover important theoretical controversies and fundamental comparative and historical puzzles, it is easy to see how constructivists might look askance at

this search for causality. When Rodden notes that the “analysis of culture as a determinant of electoral institutions has lagged behind,” one suspects that Ross, Migdal, and Blyth would agree.

Isabela Mares shows how thinking and working with causal mechanisms have generated valuable research on the welfare state. Early work in this field – statistical analyses of the relationships among economic development, openness to international trade, and the size of the welfare state – displayed strong but contrasting findings. It also began with powerful discursive analyses that linked the reaction against unregulated markets to the foundations of contemporary political economy. Following these initial claims, the literature explored many causal structures behind the welfare state: levels of income/economic development; trade openness – aggregate levels and volatility; labor market transformations associated with industrialization; electoral institutions; size of the labor force in industry; domestic market size; relative abundance or scarcity of labor; asset inequality; and developmental model/strategy – import substitution industrialization versus export expansion.

Research into the causal mechanisms behind these relationships led researchers to disaggregate the dependent and independent variables. Scholars moved from studying aggregate levels of welfare state spending to studying spending categories across programs. They also began to look more closely at the design, content, and implementation of programs – for example, levels and types of coverage and modes of financing. Along the way, the field developed important typologies of systems of social protection: liberal, conservative, and social democratic welfare states/regimes; and coordinated market economies and liberal market economies. Researchers also probed dynamics: the origin or onset of the welfare state, its endurance and persistence, and the state’s adjustment, retrenchment, and reform. Furthermore, the research domain expanded to include the study of welfare politics and politics in developing countries.

Focusing on critical causal mechanisms also led comparativists to “trace out the causal processes that are implied by existing theoretical explanations.” Scholars puzzled over “the leap of faith” between independent and dependent variables and hence concluded that “more studies exploring the intermediate steps of this causal relationship are needed.” Researchers often suggested that more than one long causal pathway lies behind regression equations, and so observable implications of competing causal processes were examined. Causal relationships thus led researchers to explore underlying causal mechanisms and processes, and underlying mechanisms and processes led them to new causal statistical analyses.

Thinking and working with causal mechanisms has also induced researchers to present more precise accounts of the political processes that produce policy outputs. The literature explores how “preexisting policies and institutions mediate demands for social protection resulting from higher levels of insecurity.” The literature also investigates the causal agency of pivotal socio-political groups, especially the working, middle, and business classes (firms). Hence, comparison need not begin with a causal hypothesis; it can also begin with a causal agent. In studying the mechanisms by which a group influences the

adoption of social policies, researchers have explored the sources of its policy preferences and interests, ideas and beliefs, organization and coherence, and the critical cross-class coalitions and alignments that generate political action.

Indeed, these chapters underline that uncovering causal mechanisms and processes now defines the center of comparative politics. At the same time, they show that ideas and methods taken from rationalist, culturalist, and structuralist paradigms help comparativists propose new causal mechanisms and pathways and help fashion new statistical studies of causal relationships. While comparativists are taking causality and constructivism more seriously, one wonders whether these two trends will be reconciled. To address this question, we turn to the study of ethnicity, the field of comparative politics where the two trends have most clearly met.

The Future of the Center: Constructivism and Causality

Is ethnicity causally relevant to political violence, patronage politics, the destabilization of regimes, the decomposition of states, and a dozen other phenomena that interest comparativists? It depends on what you mean by “ethnicity,” suggests Kanchan Chandra. Arguing that “we cannot make reasonable causal claims about ethnic identity without first defining the concept of an ethnic identity,” Chandra maintains that “without a definition, we cannot justify the inference to the role of ethnicity. Indeed, without a definition, the inference that ethnicity matters is as (un)justifiable as the inference that ethnicity does not matter.”

Suppose we could locate what Chandra calls the “intrinsic properties of ethnic identities,” the “implicit defining principles” and “primary properties” or characteristics of “ethnicity” that distinguish it from “nonethnicity.” Comparativists could then agree on which identities to classify as “ethnic” and which to label “nonethnic.” After we use the definition of ethnicity to create a relevant universe of cases, causal mechanisms that are thought to bring about dependent variables can be attributed to one category (ethnicity) rather than the other (nonethnicity). Moreover, the definition of ethnicity would help us establish which causal mechanisms ethnicity sets in motion: A well-defined concept of ethnicity entails the start-up conditions that produce dependent variables. A defining property or principle of ethnic identity is thus causally relevant, via the appropriate mechanisms, to some dependent variable; that which is not a defining property or principle of ethnic identity, but rather a property of “any identity, ethnic or otherwise,” and hence is a contingent and thus secondary property, is not causally relevant to the dependent variables of concern. In short, conceptual analysis offers a fertile heuristic for generating and sorting causal claims.

Chandra attempts to discover a generative definition of ethnicity by combing the literature: “[C]onstructing a definition that captures previous usage as far as possible is essential to moving ahead in a cumulative fashion.” Since she believes that the “essential” properties of ethnicity are “conventional” to the community of comparativists, ethnicity in both the “real” and the academic worlds are

constructed. Since she believes that there is no “objective” meaning of ethnicity, any “classification between ethnic and nonethnic identities is arbitrary.”

Chandra’s mining of the literature yields a sample of diverse causal claims. For example, ethnicity→violence, via the security dilemma; ethnicity→patronage politics, via exclusive coalitions; and ethnicity→decomposition of states, via territorial concentration. Explanation sketches of the claims reveal that each assumes that ethnicity is characterized by a particular defining property. For example, if we look at the set of explanation sketches for internal wars, we find that violence has been associated with the following characteristics of ethnicity: fixedness, the high costs of changing group membership, the visibility of group membership, distinct emotional responses, dense social networks, territorial concentration, common history, shared norms, and joint institutional affiliations, as well as, more generally, traditions, myths, and cultures. Since these properties have also been associated with the internal wars and violence of all identity groups, including women, workers, and students, they cannot underlie the specific ethnicity-violence proposition, only a general identity-violence hypothesis. To show that ethnicity matters to violence and internal wars, comparativists need a definition of ethnicity that posits a set of properties that set causal mechanisms *in ethnic groups* in motion, not causal mechanisms that operate in all identity groups. This definition would allow researchers to create a sample of cases that fit the definition–mechanism linkage, where we expect to find ethnicity and violence, and to create another set of cases that do not fit the definition–mechanism linkage, where neither ethnicity nor violence is expected.

After reviewing nearly a dozen explanation sketches of different dependent variables, Chandra sorts through the possible properties of ethnicity. Is the ethnicity (E) that produces $E \rightarrow Y_1$, $E \rightarrow Y_2$, and $E \rightarrow Y_3$ the same ethnicity (E)? What is the common core of ethnicity that, perhaps in combination with secondary properties – $E + E_1 \rightarrow Y_1$, $E + E_2 \rightarrow Y_2$, and $E + E_3 \rightarrow Y_3$ – produces the set of dependent variables? She concludes that “there are so far only two intrinsic properties that we can associate with ethnic identities, on average: (1) the property of constrained change in the short term and (2) the property of visibility.” Chandra thus settles on two descent-based attributes of ethnicity that “capture the conventional classification of ethnic identities” in the discipline. These are constitutive properties – they construct ethnicity rather than presume it – and they are distinguishing properties – they separate ethnicities from other identities. To further establish whether these defining characteristics of ethnicity match comparativists’ conventional understandings of the concept, Chandra assembles another set of cases. Using “a sample of identities that most comparative political scientists agree are ethnic,” for example blacks and whites in the United States and Serbs and Croats in the former Yugoslavia, she “show[s] that her definition captures” the elements in her sample.

Confident in the conventional validity of her definition, Chandra uses the definition to conclude: “Only a handful of our causal claims rest on the intrinsic properties of ethnic identity.” Rather, “by far the largest number of explanatory claims about ethnicity rest on properties that I have argued are not intrinsic

to ethnic identities,” for example fixedness, common history, dense social networks, distinct emotional responses, institutional ties, and spatial concentration. “As such, they cannot be taken as reasonable claims about the effects of ethnic identities in general. They should be reformulated as claims about a specific subset of ethnic identities, or claims about the effects of ethnic identities combined with some additional variable.” For example, territorially concentrated ethnic identities – not all ethnic identities – generate patronage politics and internal wars.

Chandra’s insights into causality were stimulated by constructivist challenges to mainstream studies of ethnic politics. To Chandra, an essential property of ethnicity is a conventional property as seen by scholars. To others, if ethnicity – defined by constrained change and visibility – is not something real and concrete in the world, if the definition does not distinguish between ethnic/nonethnic identity dichotomy “at the joints,” but only synthesizes current social scientific thinking; and if the ethnic category does not have the “intrinsic” properties of a natural or social kind, then “ethnicity” contains an arbitrary collection of cases that does not constitute a coherent research domain. If one manages to create a sample of ethnic and nonethnic groups based on an academically conventional principle, one will find that the ethnic/nonethnic distinction has no causal impact on internal wars, public good provision, and so on, because the independent variable is too causally heterogeneous. In other words, if they are to serve as the basis of causal claims, definitions must point to what has been “really” constructed by social actors. As Blyth indicates, ideas have real impacts. And perhaps it is the ideas associated with ethnic groups – their values and beliefs – that are the key causal mechanisms. If so, a general “guideline” for an ethnicity/nonethnicity categorization “closes the door on some” significant causal claims even as the disaggregation of ethnicity into multiple properties reveals “many tens of precise” causal pathways.

In popular and journalistic parlance, ethnicity includes “religion, sect, language, dialect, tribe, clan, race, physical differences, nationality, region and caste.” Chandra’s important contribution to the literature on ethnicity is to demonstrate that this heterogeneous set of categories may not generate the causal mechanisms required to make effective causal claims that ethnicity matters. In “placing causal theorizing about ethnic identity on a [firm] conceptual foundation,” and in showing how analytical classifications and causal theories are connected, Chandra foretells the next decade of research in which comparativists attempt to connect constructivism and causality.

FINAL WORDS

Our chapters recognize that comparative politics is a field of many paradigms and methods. “Creative thievery” between different theoretical accounts, as Anderson calls it, or their “productive friction,” in Rodden’s words, invigorates contemporary comparative inquiry. Statisticians pay attention to case studies and field workers to stylized regressions. Rationalists learn from structuralists who read culturalists who consume what rationalists produce.

Reviewing our chapters shows that during the past decade or so, causal analysis has pushed the research paradigms from the center of comparative politics. The science of comparative politics is causality all the way down and all the way across. Comparativists always compare, and comparison leads them to causal mechanisms and processes that fill the gap between independent and dependent variables. The conceptual analysis of terms, observable implications of ideas, plausibility probes into arguments, multiple outcroppings of hypotheses, and the triangulation of methods on concrete cases lead comparativists to ever-more-causal grittiness. Comparativists want to read about intellectual debates, but not at the cost of advancing causal explanations of the world.

Our chapters thus tell a story about comparativists moving beyond the paradigm wars Lichbach describes and pragmatically engaging the issues of causality that Zuckerman discusses. As comparativists drill down from transcendent concepts, timeless and spaceless claims, and abstract assumptions about human nature, causal pragmatism seems to dominate. As comparativists move away from concrete comparisons, the role of paradigms looms larger. Big-picture pragmatism holds the ideographic and nomothetic threads together, encouraging vitalizing frictions among approaches. In spite of challenges from specialists in quantitative and qualitative methods; in spite of demands from rationalists and structuralists, and especially from culturalists and constructivists; and in spite of having more methods than results, for now at least, the messy center of comparative politics holds.

“The founders of the SSRC [Social Science Research Council] Committee on Comparative Politics more than a half-century ago have reason to smile,” writes Katznelson. Optimism and excitement indeed pervade our chapters. We do have a single field of inquiry. Contemporary comparativists find their field a fruitful and exciting mixture of grand vision and pragmatic explanation.

Thinking and Working in the Midst of Things

Discovery, Explanation, and Evidence in Comparative Politics

Mark Irving Lichbach

I am never precisely sure where I am going or whether I will ever arrive. I write from the midst of things and I cannot see their order as yet. . . . I want to run but I can only drag along slowly.

Alexis de Tocqueville

INTRODUCTION

Comparison always helps. Consider what Robert B. Pippin (2005: 14) writes about philosophy departments:

Why is it that philosophy departments with some range of ages among the faculty can look like a historical zoo of some sort, with “species” from various periods locked into programs that have long since ceased to generate articles, books, dissertations? The ordinary language species here, the Rawls-Nozick person there, the deontic logic type here, the Kuhnian there, the intentional inexistence specialist laboring away in this corner, the transcendental arguments skepticism-refuter over there? . . . [F]or the most part traditional philosophy aspires not to have a history like that of literature or art (where times and places seem profoundly relevant) but a history like chemistry (where they do not; no serious working chemist worries about the history of chemistry). But the . . . labels stick nonetheless and have become part of the daily discourse of philosophy.

Skeptical about coherent doctrines that define the history of ideas (Skinner 1969), John Dryzek, Bonnie Honig, and Anne Phillips (2006: 5, 6) introduce an 883-page survey of political theory with a somewhat different set of observations:

Political theory is an unapologetically mongrel sub-discipline, with no dominant methodology or approach. When asked to describe themselves, theorists will sometimes employ the shorthand of a key formative influence – as in “I’m a Deleuzian,” or Rawlsian,

Earlier versions of this chapter were presented to the panel on “Rationality, Culture, and Structure in Comparative Politics: Classic Themes, New Directions,” at the annual meeting of the American Political Science Association in Philadelphia, August 30–September 3, 2006, and to the conference on “Rationality, Culture, and Structure in Comparative Politics,” in Bellagio, Italy, October 16–October 20, 2006. I thank the participants for their valuable feedback. Ira Katznelson, Margaret Levi, and Alan Zuckerman helped refine my message.

or Habermasian, or Arendtian – although it is probably more common to be labeled in this way by others than to claim the description oneself. In contrast, however, to some neighboring producers of knowledge, political theorists do not readily position themselves by reference to three or four dominant schools that define their field. . . . In the last two decades of the twentieth century, liberal, critical, and post-structuralist theorists have (in their very different ways) . . . rethought the presuppositions and meanings of identity, often rejecting unitary conceptions and moving towards more pluralistic, diverse, or agonistic conceptions in their place.

Is Pippin also describing *our* meetings? Or, following Dryzek, Honig, and Phillips, are various species of rationality, culture, and structure no longer hired to sit around *our* conference tables? In other words, has comparative politics moved beyond a battle of the paradigms?

This chapter, which is about alternative ways of thinking and working in comparative politics, evaluates two competing claims about the current state of our field.

Claim: Comparative Politics as Chemistry and Physics. Discourse in comparative politics is nowadays postfoundational. Since many comparativists – former rationalists, culturalists, and structuralists – deny that social scientific truths are relative in terms of the history and interests of a theoretical language, they avoid debates over transcendent metaphysical fundamentals. “Permanent foundational debates . . . are a reliable indicator of the immaturity of a discipline” (Callebaut 1993: 3, fn 11). Epistemology and ontology are out; methodology – the vocational training of toolboxes and cookbooks – is in. Paradigm warriors and theoretical fundamentalists are shunned; paradigm busters and synthesizers are embraced. Comparative comparativists – those interested in comparing concepts and cases – have therefore turned to the pragmatics of discovery, locating significant problems and puzzles; the pragmatics of explanation, generating middle-range causal mechanisms; and the pragmatics of evidence, deploying historical narratives and robust statistics. Today’s practical comparative politics recognizes no such thing as a paradigm.

Counterclaim: Comparative Politics as Literature and Art. Pragmatism works in theory but not in practice.¹ The absence of foundations is itself a foundational claim, and the self-referential incoherence of the admonition to “always pragmatize” recalls Fredric Jameson’s (1981) recommendation to “always historicize.” The search for contexts of discovery, patterns of explanation, and styles of evidence is really about the search for foundations. After all, where do comparativists find their “best practices,” those particular “systematic patterns of scientific thinking [that] may produce much more rapid progress than others” (Platt 1966: 19)? Because they return to paradigms as foundational heuristics, pragmatic comparativists are inevitably parasites living off the paradigm warriors they shun. In today’s comparative politics, then, paradigms still rule.

¹ A joke attributed to the late Sidney Morgenbesser, professor of philosophy at Columbia University.

This chapter argues that comparativists, like most moderns, are skeptics adapting to lost foundations. Students of politics seem impelled to borrow from disciplines that specialize in more ultimate realities. Hence, rationalists turn to economics and psychology, structuralists to history and sociology, and constructivists to anthropology and literary theory. Although I chronicle the rich variety of ways that comparative politics has been pushing beyond this simple three-grid way of thinking and working, and although I show that comparativists now grasp a messier but fuller reality, I also clarify how paradigmatic distinctions still inspire synergetic work. Even the most creative syntheses have not erased the fruitful distinctions among paradigms. Rationalist, culturalist, and structuralist practices still stimulate internal developments within paradigms and generate all sorts of interesting and surprising border crossings. Since comparativists continue to think and work via models and foils, comparative politics remains a field of multiple paradigms, and the tensions among its paradigms create some of its most valuable dynamics (Lichbach 2003).

The chapter opens with a short genealogy of paradigms in comparative politics. I trace our current crisis of understanding to the behavioral revolution of the 1960s,² show how paradigms were embraced as the philosophers' stone that would remedy the deficiencies of variable-based reasoning, and then discuss the revolt against paradigms. The bulk of the chapter discusses approaches to the messy center of comparative politics, offering exemplars of pragmatic approaches to discovery, explanation, and evidence that attempt to eliminate paradigms and yet still anchor research. I then return to the debate about paradigms and explore the argument that paradigm thinking produces deadening metaphysics and the counterargument that paradigm thinking facilitates vitalizing frictions. My conclusion is that synthesis is occurring but that creative research derives from thinking and working with paradigms in the midst of things.

ORIGINS OF THE PRESENT CRISIS OF UNDERSTANDING

Let us place the current era in perspective. Late-twentieth-century and early-twenty-first-century comparative politics, that is, modern comparative politics, began with the positivism of the 1960s. All else has been embellishment, and all since the beginnings is disestablishment.

While the new political science of the 1960s was called "behavioralism," it is better understood as a semantic and syntactical language for variable-based thought. Comparativists began with a conceptual framework of terms. The neo-Alexandrians compiled inventories of key variables. For example, Beck's (1975) American Political Science Association (APSA)-sponsored *Political Thesaurus* and Robinson, Rusk, and Head's (1968) Interuniversity Consortium for Political and Social Research (ICPSR)-sponsored measures of political attitudes consisted of

² I use the terms "research communities," "traditions," "languages," "schools," "approaches," "paradigms," "programs," and "frameworks" interchangeably. Lichbach (2003) provides definitions, and Dow (2004) reviews a similar debate over paradigms in economics.

lists of conceptual and operational definitions. Others created consortiums of datasets, and still others produced propositional inventories, if-then statements culled from articles and books (Lasswell and Kaplan 1950; Eckstein 1980a: 6). The encyclopedists who organized sets of plausible rival hypotheses required a bit more architecture. They mined their gold and classified their jewels with a static levels-of-analysis framework (e.g., international relations theorists such as Waltz 1959 and Singer 1961) or with a dynamic funnel of causality (e.g., American political behaviorists at the University of Michigan). The statistical upshot of such efforts was hypothesis testing and standard causal analysis: gross input/output studies that assessed causality in terms of simultaneous equation models.

Such abstract empiricism, often expressed in a highly technical idiom that went beyond ordinary language usage, presupposed and indeed came to reproduce a set of assumptions about social reality: Idealized yet indirectly observable social forces and properties have an impact on other equally hypothetical social forces and properties. The result is measurable correlations, a sort of transcendent glue bonding the social forces and the properties. Causality is thus a predicate of statements about a world of fixed and concrete entities with variable and abstract attributes. The purpose of social theory, then, is to explain a phenomenon by finding its cause. The strategy of the social theorist entails reexpressing complex social dynamics with a theory filter of variables (independent, controlling, dependent) and with a research design screen that can establish the importance and significance of the variables. For example, when one says that the X variable of education causes the Y variable of income, one is employing a quick and dirty – but efficient – way of summarizing many plausible narratives and stories in which X accounts for Y. To determine if one's account has validity, the researcher evaluates it against the usual suspects, a long, eclectic list of variables, causal and control, taken from various scholarly and popular camps. To investigate one's own pet theory and those of others, relevant variables are then tossed into a canned linear regression (nowadays a maximum likelihood [MLE] package). In the empirical domain under investigation, one can thereby hold "everything else" constant and compare the conditioning effects of the plausible rival hypotheses on the distribution of the dependent variable. If several dependent variables are interrelated, one repeats the exercise and creates a causal diagram of direct and indirect effects. By determining how heretofore unrecognized patterns or shapes in the distribution of a variable are pushed around by regressors, social science, like mathematics, makes the invisible visible. If one's pet variable loses the contest and if, for some reason, one cannot switch to another pet variable, Popper's (1965) widely accepted falsification criteria dictate that this too is science.

What is wrong with this picture of the science of comparative politics? Is it not the worst approach to social inquiry, except for all the others that have been tried over time? Critics charge that its patterns of evidence, explanation, and discovery never satisfy.

Statisticians complain about the evidence. Garbage can or kitchen sink regressions produce tables of regression coefficients that are long and

uninterpretable. Unstable and nonrobust across studies, they are unusable as predictors. Since the counterfactuals – “if only” or “what if” alternatives – are too complex and the black box statistics are not transparent, the results are simply not believable. Like climate scientists who cannot distinguish global warming from global cooling, comparative social scientists cannot even get the sign right. Because comparative politics is a science better at “knowing that” than “knowing how,” the accumulated statistical conclusions of statistical comparative politics are not cumulative.

Theorists, by contrast, complain about the explanation: Correlation is not causation. “Learning about many things does not produce understanding” (Heraclitus, cited in Nussbaum 1997: 85). When comparativists “slice and dice . . . cases into variables” (Katznelson 1997: 89), “positive and negative factors run amok” (Eckstein 1980b: 138). “Theory as a sum of variables” (Sørensen 1998: 247) thus results in “evidence without insight.” In other words, the “distinctive modernist empiricism that sets out to atomize and compartmentalize the flux of reality and to develop new approaches to the gathering and summarizing of empirical data” fails (Adcock, Bevir, and Stimson (2007: 9). Alan Zuckerman’s (1997) chapter in the original Lichbach–Zuckerman volume challenged positivism’s analytic eclecticism. Put simply, linear correlation/covariance studies do not automatically address *why* and *how* questions. Gross input–output studies, which display networks of relationships among variables as forces, are uninteresting. If comparativists want to know how to get from here to there, they need to go beneath the variable world and probe the synapses – the spaces between causes and effects. In other words, they need to know what undergirds observed relationships and what principles or things are depicted by statistical equations. Variable-based comparative politics explores obvious surface regularities and therefore never makes the invisible visible. A true social science seeks patterns that are hidden and deep.

These difficulties of evidence and explanation have led some comparativists to argue that the variable-as-force ontology does not lend itself to identifying substantively relevant problems. The approach is too methods driven and is insufficiently problem oriented. While social scientists debate their methods, physical scientists debate their results, or as Poincaré (2001: 366) puts it: “Nearly every sociological thesis proposes a new method, which, however, its author is very careful not to apply, so that sociology is the science with the greatest number of methods and the least results.” Writing around the same time as Poincaré, Weber (1977: 11, emphasis in the original) cautioned that “sciences are founded and their methods are progressively developed only when *substantive* problems are discovered and solved. Purely epistemological or methodological reflections have never yet made a decisive contribution to this project.” In fetishizing method, variable-oriented analysis is based on the law of the hammer and represents the worst form of pretentious scientism. Eckstein (1980a: xiv) complained that exercises in technical sophistication, quick and dirty procedures that forge mechanical combination of variables, yield a poor motivation for social science “that too often makes the development of ideas appear to be a

merely technical matter: one of grinding out work according to procedural canons, like those of statistics. One of the more harmful results of this is that theory comes to be conceived as the product of manipulated data, not something that, especially at inception, involves poetry and vision. A consequence of that is, and can only be, prosaic ideas – ideas that never penetrate surfaces.” Critics thus find variable-based comparative politics to be laborious, pedantic, tiring, trivial, uninspired, dull, dreary, thoughtless – need I continue? – a normal science that we do not need.

The variables-as-forces approach was enshrined for comparative politics in Przeworski and Teune’s (1970) classic maxim: Substitute names of variables for proper names of political systems. Abstract features, attributes, and properties of countries explain other equally abstract features, attributes, and properties of countries. But here’s the problem in a nutshell: To explain why the rate of heart attacks in the United States is greater than that in Japan, one can begin, but not end, by comparing distributions conditional on per capita consumption of saturated fat. Given the positivist’s explanatory scenario, the crises of variable-based comparative politics were thus predictable (and indeed were predicted). While area studies were plagued with large V and small N – too many variables and too few cases – cross-country regressions were initially plagued by small V and large N – we lacked data on central causal factors. Some four decades later, comparativists have assembled many different types of information, including regional, organizational, and individual data, on 180 countries for nearly 50 years. Nevertheless, in the eyes of many, large-V and large-N studies turn out to be the worst of both possible worlds, only exacerbating the difficulties of discovery, explanation, and evidence just discussed.

The First Attempt to Creatively Deepen Variable Analysis Was the Search for Paradigms. If thinking and working in comparative politics involved more than variable-centric theorizing, perhaps comparativists’ ontology, epistemology, and methodology needed rethinking. Perhaps, unlike economics but like sociology, political science and comparative politics were multiple-paradigm fields of inquiry. The early paradigmists in comparative politics thus stressed the “theoretical power of multiple paradigms” of inquiry (Alford 1998: chap. 7). Seeing the advantages of moving beyond variable analysis, they expected to employ a well-equipped toolkit of theoretical explanations and empirical methods that could solve important problems.

In the first edition of this volume, Lichbach (1997a) suggested that the structure–culture–action nexus was at the core of social science. While the rational choice tradition studies how agents with preferences and beliefs interact and thereby produce outcomes, the structural tradition explores how principally material conditions more directly produce these outcomes. Social construction is the third way between structure and action: Since structures condition actions and agents create structures, constructivists explore the coevolution of structures constituting individuals and individuals constructing structures. The middle ground of inquiry thereby consists of intersubjective or social facts – for example

language, rituals, and laws – that are not reducible to individuals. Comparativists tend to coalesce into research schools that study these different aspects of social order. Problem domains in comparative politics, such as political economy, voting and elections, institutional choice, and conflict studies, contain multiple research traditions and their associated theories and methods. Different conceptual vocabularies have become associated with the different paradigms.

Rationality	Culture	Structure
bargaining	identity	regime
collective action	difference	capitalism
credible commitment	membership	state
incomplete information	boundaries	society

For definitions, illustrations, and elaborations of paradigms and of rationality, culture, and structure, see Lichbach (1997a, 2003).

With the legitimacy of paradigms seemingly assured, during the 1980s rationalists, culturalists, and structuralists fought for “separate tables” in comparative politics (Almond 1988). Popkin (1979), Bates (1981, 1983, 1989), Przeworski (1985), Ames (1987), Levi (1988), and Tsebelis (1990) led the onslaught of rational choice theory against the positivist establishment’s way of thinking and working. Skocpol (1979, 1985) pioneered a theoretically and substantively rich statist perspective that eventuated in historical institutionalism. The early culturalists, who have been rejected by present-day constructivists, fought back hard (Chilton 1988; Wildavsky 1987, 1988; Inglehart 1988; Eckstein 1988).

Thus, within a single decade, comparative politics saw the appearance of a variety of competing paradigms. Many comparativists, following Kuhn (1970) or Lakatos (1970), concluded that they needed an approach or perspective on the field. Its common vocabulary would describe things, its language would explain why things happen, and its nomenclature would uncover deep understandings about the operation of phenomena.

As rationalists, culturalists, and structuralists developed their ideas within the self-imposed confines of a particular framework, each asked, how would a *true* rationalist argue? What is the *quintessential* structuralist argument? What would a *real* culturalist theory look like? A succession of paradigm warriors (Tarrow 2004) in comparative politics thus stressed each perspective’s carefully delimited but critically distinct themes and tools.

Since languages differ from one another, it looked as if the center of the field did not hold. With the field deconstructed into its intellectual parts, it appeared that there was no one such thing as comparative politics. Given that truth was relative to perspective, the “ism” wars – paradigm wars among comparativists who held strong priors about theory – had broken out. And the fight was as much about scholarly identities as about scholarship.

The first edition of this book, which appeared in 1997, followed nearly two decades of dogmatic sectarian proselytizing. By that time, most comparativists

had become uncomfortable with the battle. As Przeworski and Teune (1970) argued almost 40 years ago, comparativists compare states and thus necessarily combine structure and action or context and process. Rather than an essentialist struggle for explanatory power, comparative politics is about the interrelationships of rationality, culture, and structure. In an important symposium published in *World Politics* in 1995, the participants thus groped for the “messy center” of the field. Comparativists, it was argued, generally loathe a battle of the paradigms and strongly favor a field with a single transparadigmatic set of methodological tools or explanatory standards, in other words, a ground between dissected variables and holistic paradigms, that can operate in the midst of things. My colleagues’ trepidations, as noted in Lichbach (1997a), were prescient. Perestroika – a dissident movement – was soon organized by those who feared that statistical-oriented and rational choice-driven political scientists were winning the battle and capturing the discipline (Monroe 2005). The authors in our first edition had exposed a raw nerve that soon became an open sore – a clear and present danger to the academic integrity of the field.

Leading scholars were also mobilized and sought to transcend the perils of perestroika. Asking “How can we comparativists all get along?” they advocated a transparadigmatic dialogue denying that comparative politics is inherently or essentially a multiparadigm field.

Two fin de siècle articles that compared different types of new institutionalisms (rational choice, historical, and sociological) solidified this antiparadigm thinking. When Immergut (1998) located their common foil – behavioralists – she united rationality, culture, and structure. After Thelen (1999) further isolated important commonalities among new institutionalisms, many thought that a key intellectual debate had been settled empirically. Subsequently, Campbell and Pedersen (2001) located what they saw as a second movement in institutional analysis: a dialogue among the new institutionalisms. On the rational choice side, Levi’s (1997b) chapter in our first edition pointed to commonalities, as did Bates, Greif, Levi, Rosenthal, and Weingast (1998a) and Bates, de Figueiredo, and Weingast (1998b). Meanwhile, empirical studies of institutions, such as Bunce’s (1999) analysis of the “subversive institutions” of state socialism, linked institutions, interests, and identities.

The field’s new ideas about methodology were being shaped by its new perspectives on paradigms. In the culmination of a series of developments at such elite schools as Harvard and Stanford, Laitin penned what I can only call a paradigmatic statement about the future of comparative politics. He (2002: 630, 659) proclaimed that “[t]oday, a new consensus is on the horizon, one that emphasizes a tripartite methodology, including statistics, formalization, and narrative. . . . In comparative politics, interdependence across the tripartite methodological divide, with grudging toleration built on mutual suspicion of practitioners across the divide, is a key to scientific progress.” Graduate students thus should be trained in statistics + formal theory + narratives. By common understanding, case study narratives for young comparativists grow out of carefully crafted dissertation-based fieldwork. If Marxists fanned the paradigm

wars in the 1960s by asking behavioralists, “What’s your theory of the state?” methodologists cooled the wars in the 1990s by inquiring, “What’s your research design?”

Within a couple of decades, the thesis of paradigm wars begot the antithesis of a disciplinary center. While each (half) generation claimed to have overthrown and revolutionized the tradition it had inherited from its elders, the elders claimed to have reformed the tradition they had once fashioned. In this dialectical-historical-generational struggle, a booming academic job market meant that everyone could win the race and therefore everyone could take home a prize.

The next three sections of this chapter analyze these recent developments in comparative politics. I study 10 transparadigmatic alternatives for structuring inquiry, for thinking and working in comparative politics without paradigms. While some are rather new, others reinvent valuable wheels. Divided among approaches to discovery, explanation, and evidence, I will praise them as practical strategies that face acute difficulties but nevertheless allow comparativists to get along.

Discovery

- big problems
- thorny puzzles
- core difficulty

Explanation

- big concepts
- mechanisms
- institutions
- middle range causal arguments

Evidence

- stylized facts
- designs for establishing causality
- analytic narratives

DISCOVER A DIFFICULTY

“It is so difficult to find the *beginning*,” writes Wittgenstein (1972: 62e). “Or, better: it is difficult to begin at the beginning. And not try to go further back.” The Hebrew Bible famously begins at the beginning, yet to avoid a false chronological foundation, one translation (Stone Edition *Tanach*) employs two participles: “In the beginning of God’s creating.” Knowing that discovery is all around him, Tocqueville (cited in Wolin 2001: 86) begins in medias res, “in the middle of things.”

Nevertheless, some social scientists seek a fixed Archimedean point or God’s-eye view. They claim to begin at the start and work forward, or to adopt the backward-induction idea and begin at the end and work toward the starting point. A statistician of the first rank, Paul Lazarsfeld (1982: 208), responds with a line from T. S. Eliot, “. . . the end of exploring/Will be to arrive where we

started/And know the place for the first time,” and a famous logician, Bertrand Russell (1985: 49), writes that “I do not pretend to start with precise questions. I do not think you can start with anything precise. You have to achieve such precision as you can, as you go along.” Even accomplished scholars work out their true purposes and uncover their real questions only in the vortex of inquiry. As many prefaces admit, “This is not the book I intended to write” (Smith 2005: xiii). Kuhnian normal science is thus a beginning that operates, like the Bible and Tocqueville, in the midst of things. According to Kuhn, the basic unit of scientific analysis is not the hypothesis, theory, or bit of evidence but rather the solved problem. His rational reconstruction of historical cases of creativity uncovered the applied models and crucial exemplars that were the heuristics or rules for discovering problems. In other words, science, like any community, operates against a background of claims, practices, and methods considered settled. Science, like all traditions, looks backward in order to move forward. To address the problem-solving potential of rational choice theory, constructivist theory, or structuralist theory, Kuhn thus teaches us to ask, “What problems have already been solved?”

Kuhn is now recognized as the pivotal philosopher of science of the second half of the twentieth century. Those elements of Kuhn that one appropriates reveal much about one’s approach to social science. Those who reject Kuhnian paradigms, for example, favor Kuhnian normal science, teaching their students to follow the successful exemplars of applied game theory that appear in textbooks.

Kuhn, it should be remembered, replaced the distinction between the context of discovery and the context of justification with the distinction between revolutionary science and normal science. Once upon a time, Hans Reichenbach’s distinction between discovery and justification was a crucial part of science theory. Giere (1999: 14; emphasis in the original) reminds us that the boundary was in part politically motivated: “[P]art of the significance of the distinction for Reichenbach at this time [mid-1930s] was its implicit denial that characteristics of a *person* proposing a scientific hypothesis have anything to do with the scientific validity of the hypothesis proposed. This applies, in particular, to that person’s being a Jew. Reichenbach seems to have made it a precondition on any scientific epistemology that it rule out the possibility of any distinction between, for example, Jewish and Aryan science.” Discovery was thereby banned from science studies by Hempel, who fled Hitler. If science is infallibilist, or the accumulation of true statements, then a true “logic” of discovery, Popper (who also fled Hitler) argued, is crucial. However, if science is fallibilist, or the accumulation of falsified theories, then the “logic” is less important. The history of the problem can only be “the history of some bad ideas” (Rorty 1982: 176, 177). To prevent academics from sitting *shiva* for losers and dead ends, literature reviews should be kept mercifully short: “But in fact intellectual progress usually occurs through sheer abandonment of questions together with both of the alternatives they assume – an abandonment that results from their decreasing vitality and a change of urgent interest. We do not solve them; we get

over them. Old questions are solved by disappearing, evaporating, while new questions corresponding to the changed attitude of endeavor and preference take their place" (Dewey 1973: 41).

There was another reason to avoid discovery: the formlessness of serendipity. Scientific curiosity does not follow predefined plans. Reichenbach (1951: 231) taught that "the act of discovery escapes logical analysis; there are no logical rules in terms of which a 'discovery machine' could be constituted that would take over the creative function of the genius." Newton's apple and Archimedes' bath are neither algorithms for discovery nor rules for generating flashes of insight. In the hands of scientists who can stand back and think laterally, soaking and poking – barefoot empiricism in uncharted waters – can discover previously unknown phenomena and open up new research frontiers. Genius, the unstructured and free invention of the mind, is an art and not a science. Inspiration cannot be learned; it is innate, a special gift of talent or creativity that comes from treating tools as toys. *Pirkei Avot* (*Ethics of the Fathers*) reminds us that "a fool can ask more questions than nine wise men can answer." Kant writes that "[i]t is a great and necessary proof of wisdom and sagacity to know what questions may be reasonably asked" (cited in Rickman 1983: 119). A good question, it is said, already contains half of the answer.

Scientists therefore cannot routinely attach "why" to a preconstituted and already meaningful phenomenon and arrive at a significant problem. If finding, recognizing, and formulating problems were that easy, a statistical compendium or historical chronology of dumb facts – things-in-themselves – could automatically lead to the discovery of problems worth tackling. However, science constructs its domain of facts and objects as classes or kinds of things that are suitable to its theories and methods. Social phenomena are thus social scientific objects whose meaning and boundaries are constituted by social science in ways that can be studied in replicable investigations. The implication, as Popper (1976: 135) notes, is that "[p]roblems are not easily identified or described, unless, indeed, some ready-made problem has been set us, as in an examination; but even then we may find that the examiner did not formulate his problem well, and that we can do better. Thus there is only too often the problem of formulating the problem – and the problem whether this was really the problem to be formulated." Merton (1968: ix, x) thus elaborates the importance of finding what he calls a "generating question of inquiry": "At first appearance, it would seem fairly easy to see and pose a problem in a branch of science. Surely the raising of questions presents no great difficulty; children do it all the time. And yet, the experience of scientists is summed up in the adage that it is often more difficult to find and to formulate a problem than to solve it. . . . Although every problem in science involves a question, or a series of questions, not every question qualifies as a scientific problem." Katzenstein (1995: 11) concurs on the absence of ready-made problems: "I have learned that you have to ask important and interesting questions. That is the hardest thing to do and the hardest thing to teach. We can teach paradigms, analytical perspectives, and methods by taking them off the professional shelf and transporting them to the classroom. What we

cannot teach so readily is how to ask important and interesting questions, the hallmark of superior research and great teaching.”

Since fruitful points of departure are treasures, I tell my students that they should think long and hard (but not endlessly!) about what is questionable about a problem: Who thinks it’s a problem, and why does it matter to them? What are the subproblems, and what would solutions entail? I then add that unless research is properly motivated, it is unlikely to make a contribution. To press my point, I suggest that they consult any textbook on research design. All of them remind young researchers to establish a clear statement of significant study objectives. While romantics can say that unstatable questions are the only important ones, social scientists work toward clear specifications of problems. Normal science thus looks paradigm free. Pragmatic problem solving, moving from the indeterminate and discordant to the stable and coherent, seems to dispense with paradigms and allow scientists to appraise the merits of different theories (Laudan 1977).

Many comparativists indeed hope for a logic of discovery that can deepen variable analysis and yet not suffer the pitfalls of a battle of the paradigms. They aim to motivate research so that a carefully constructed “problem situation” – Popper’s (1965) term for the “midst of things” – offers a background of current explanation and evidence that engages readers from different paradigms and permits them to critically evaluate new problems and solutions. Borrowing from Popper (1965: 159), comparative politics begins and ends with problems, and the history of comparative politics is the history of its problems. How then can comparativists tackle genuine problems? For contemporary comparativists,

The Problem Situation = Big Problems + Thorny Puzzles
+ the Core Difficulty.

Discovery = Big Problems

Perhaps there are universal, that is, transhistorical, issues that define fields and disciplines. Candidates in political science include

- conflict and contention: who gets what, when, and where;
- stratification and domination: who gets nothing;
- justice and exploitation: the trade-off between efficiency and equity;
- distribution and redistribution: the authoritative allocation of values;
- power and order: the legitimate monopoly of coercion;
- competition and participation: how people influence authority structures;
- and the persistence and change of regimes: evolution and revolution.

Shapiro et al. (2006) report that Yale’s big-problem initiative focuses on repression and popular rule; order, conflict and violence; identities, affiliations, and allegiances; crafting and operating institutions; and distributive politics. Endless tomes opine on the rise and fall of empires, the clash of civilizations, and the subjugation of women.

Here, there, and everywhere? A more fruitful approach to discovery looks to the historicist cure. Since neither politics nor political scientists always work in the same way, there are no perennial problems. New times require new understandings of old issues, constructivists remind us, and our ideas have no fixed meanings. Hence, we must always historicize (or, to avoid self-referential incoherence, always politicize) and appreciate the origins, persistence, and change of academic debates. Discovering problems thus requires a sense of timing and of specificity: Big problems are important yet unanswered contemporary questions about real-world challenges.

Back in the 1960s, we wanted to know whether someone was serious, that is, whether that person aimed to serve something greater than his or her self and hence had something to say. Weber (1946) thus referred to “value relevance,” or a sense of world-historical significance. He urged social scientists to understand where *we* came from, thereby appreciating the vital problems of *our* age and identifying the ones *we* are fated to address. A historically specific world vision can derive, as in Weber, from a comparative study of cultures. Such study could clarify our values and establish our point of view. It might allow us to cut through our conflicting claims and interests and focus our moral passions, ethical purposes, and political commitments. Weber’s diagnosis of the impoverished modern soul, of course, hinged on the tragedy he believed had befallen the West: disenchantment, depersonalization, and alienation. He urged social scientists to overcome the disillusionment that results in moral skepticism, political nihilism, and philosophical relativism. We have no choice but to come to our own understandings of good/bad, right/wrong, and just/unjust.

One legacy of Weber is the suggestion that intellectuals should care for *res publica* – the commonweal. Comparativists must address the important policy-relevant issues of our times, engage our urgent real-world challenges, and reflect on the critical problems in our lives. We must go beyond criticism to offer a constructive critique for mending the world.

C. Wright Mills (1959: 6–7) also offered a historicist perspective on contemporary public issues: “The sociological imagination enables us to grasp history and biography and the relations between the two within society.” He raised three crucial questions: “(1) What is the structure of this particular society as a whole? . . . (2) Where does the society stand in human history? . . . (3) What varieties of men and women prevail in this society and in this period?” Mills (1959: 225) wanted social scientists to address “the terrible and magnificent world of human society in the second half of the twentieth century.”

Contemporary political scientists echo Weber’s and Mills’s views on what has come to be called “problem-driven research.” Katznelson (1997), in his chapter in our first edition, takes this point of view. Kohli (2002: 85) writes that “there are those who like to worry about how to study a problem and there are those who study problems.” He thus stresses “research problems of real-world significance . . . problem-oriented research programs” (116). Shapiro (Shapiro, Smith, and Masoud 2004) is the most determined contemporary spokesperson for the importance of problem-driven research.

Today's comparativists follow this recipe and are very much engaged politically. In order to understand the terrible history of communism in the twentieth century, Tismaneanu (2003) explores Romanian communism. Marx (1998) compares how race has affected citizenship in South Africa, the United States, and Brazil. Posner (2005) seeks the origins of ethnic identity and conflict in Africa. Baldez (2002) relates the timing of women's movements to the framing of key political issues in Chile. Tsai (2007) explores the democratic future of China's capitalist path. Lust-Okar (2005) studies how a regime's structure of contention includes and excludes political opponents, thereby affecting Islamist movements in the Arab world. Yashar (2005) explores indigenous movements in Latin America as challenges to neoliberal reforms.

No paradigm or approach has a monopoly on understanding these historically embedded problems. Beginning with a substantive problem forces the researcher to bridge epistemological divides and methodological specializations. The problem-motivated researcher uses mixed/multiple paradigms (rationality, culture, and structure), as well as mixed/multiple methods (quantitative and qualitative). Rather than thinking only of academicians and their theories, enmeshing themselves exclusively in networks of scholarly debates, comparativists should also confront the big problems out there in the world that resist explanation.

Discovery = Thorny Puzzles

Another approach to discovery adopted by comparativists sorts the questions from the puzzles. As Zinnes (1980: 318) puts it, "Puzzles are questions, but not every question is necessarily a puzzle."

The genealogy behind this elliptical statement comes from mathematics. Polya (1981: 118) writes that "[s]olving problems is the specific achievement of intelligence, and intelligence is the specific gift of man. The ability to go around an obstacle, to undertake an indirect course where no direct course presents itself, raises the clever animal above the dull one, raises man far above the most clever animals, and men of talent above their fellow man." Lave and March (1975) wrote a classic textbook about modeling that convinced a generation of mathematically oriented political scientists to engage in "puzzle solving."

What, then, is a puzzle? Perhaps it is best to begin with the subject and not the predicate. Puzzlers are not synthesizers. They do not categorize all of our knowledge in an effort to create a comprehensive taxonomy of the crazy-quilt chaos and disorder of the social world. They do not regroup the conventional and stale, reclassify familiar and routine patterns, jam everything somewhere, rationalize every idea, pigeonhole all pigeons, and generally find a place for everything in an encyclopedia of the world. If one reads books and articles and writes literature reviews and essays with the aim of synthesizing it all – the Casablanca routine of rounding up the usual suspects – one will never be truly puzzled. To continue a point worth emphasizing: Comparativists who try to identify a puzzle are not rushing to produce the grand unity that some take to be a sign of an orderly scientific mind.

Rather, the comparativist as puzzler cultivates puzzlement. Reading the literature and writing literature reviews can be puzzle-generating activities – if one looks beyond everyday problems or questions, if one attempts to fill in more than common gaps in knowledge, and if one attempts to resolve more than the usual confusions and misunderstandings that characterize science. Puzzlers embrace intellectual discomfort and seek more than the merely interesting, stimulating, or exciting. Rather, they chase after the counterintuitive departure from commonsense belief or conventional wisdom. More than the curious anomaly, they are after whatever it is that makes a puzzle truly “thorny” – the unforeseen, unanticipated, and unpredicted, as well as the long unexplained and mysterious, the irrational and illogical.

Puzzles must be startling and jarring, alien and foreign. They must seem intractable. Think of riddles, paradoxes, enigmas, contradictions, antinomies, dilemmas, or lacunae. Or think in literary terms: irony, tragedy, pathos, comedy, and farce.

Turning now to examples, they can be found throughout the discipline. Since most Syrians dismissed such claims, Wedeen (1999) wonders why the Syrian regime persisted in orchestrating grand spectacles for President Hafiz al-Azad, to the point of proclaiming him the nation’s “premier pharmacist.” If the regime understood the causes and consequences of their country’s economic decline, Kopstein (1997) asks why East German leaders never tried to reform the economic structure. In rational choice theory, puzzles include the Prisoner’s Dilemma, median voter, chaos theorems, credible commitment, adverse selection, moral hazard, tit-for-tat, neighborhood segregation, six degrees, critical mass, and so on. Grofman (2001) offers a collection of articles by comparativists who search out puzzles. By separating the origins of democracy from its persistence, Przeworski, Alvarez, Cheibub, and Limongi (2000) think they have solved Lipset’s economic development–stable democracy puzzle. Boix (2003) promptly challenges the solution and offers an alternative. Thus, the theoretical puzzle remains a puzzle and continues to attract talented comparativists.

Discovery = Core Difficulty

If comparativists who search for big problems are driven by trends, cycles, and shocks in the messy real world, comparativists who look to thorny puzzles are driven by developments in the formulaic world of theory. If Weber’s thoughts on discovery tend toward the grandest of problems – albeit historically concrete ones – Polya’s ideas on discovery tend toward the most arcane of puzzles – albeit theoretically relevant ones. It is worth comparing and contrasting two brief but deeply stimulating volumes: Ira Katznelson’s *Desolation and Enlightenment* (2003a) and David Laitin’s *Nations, States, and Violence*. What makes Katznelson’s volume so attractive are the connections he draws between history, social theory, and normative concerns. What makes Laitin’s volume so compelling are the puzzles that flow from his packaging of general problems, middle-range mechanisms, and scientific aims. Yet Katznelson also cares about science and Laitin about a

better world. And whereas Laitin is rooted in rationalist social theory and is concerned with the history of ethnicity in the modern world, Katznelson writes about the general problems and theories developed by the social science establishment. Conclusion: There must be a middle ground between problems and puzzles.³

In a series of publications dating from the early 1960s and continuing through the late 1970s, Harry Eckstein called this messy center of discovery “problematation.” Since I cannot improve on words that bear repeating, I will quote him extensively.

All research domains are a clutter of challenges and difficulties. Amateur literature reviews thus offer “an accumulation of alternative, untested, generally ambiguous hypotheses” in which “positive and negative factors run amok” (Eckstein 1980b: 135). Paradigms cannot discipline because they often take the researcher’s imagination in various directions. Sorting through the flotsam and jetsam of current science takes a very special talent:

Undoubtedly questions are matters of arbitrary interest; methodological precepts can contribute little to raising them. The ability to put them is a gift, a matter of being imaginative enough to see unresolved issues in empirical experience. It is perhaps this gift which above all divides talented from mediocre social scientists. Having few questions, or questions that do not penetrate very far or are highly conventional, is the mark of the dullard in any field. (1964: 23)

The process of theory building, needless to say, always begins with questions about experience for which answers are wanted – and raising questions, especially penetrating ones, is anything but a simple matter; indeed, it is perhaps what most distinguishes the genius from the dullard (for whom common sense, the sense of ordinary people, leaves few mysteries). It is also an ability that, conceivably, could be shaped or dulled by various modes of inquiry. (1975: 91)

To the contemporary ear, Eckstein’s comments sound elitist, stressing the sort of intellectual cultivation that only the few, with their mostly inborn special abilities, can reach. While maintaining that there is no algorithm for discovery, Eckstein does nevertheless offer a sort of path toward excellence suitable for the rest of us: critical reflection.

After assessing a question, one more step is required in problemation. This step is much more difficult to discuss abstractly than the others, but it is perhaps even more crucial to an adequate final result of inquiry. It consists of determining what problem must be solved. If a question is to be answered, one must try to discover what is really puzzling about a question and to analyze it to the point where its crucial elements are revealed. (1964: 31)

Between problems and solutions in positive inquiry there lies a crucial step. . . . The step I have in mind is the most fateful aspect of what I once called “problematation”: *the discovery*

³ Edmonds and Eidinow (2002) offer a related distinction.: While Popper wishes to solve grand problems about genuine and profound real-world concerns, Wittgenstein aims to unravel the apparent minutiae of self-imposed language games whose puzzles bewitch reason.

of the most fundamental problem requiring solution if a proper development of a theory about a subject is to occur. (1980b: 135, emphasis in the original)

The discovery of primary problems usually culminates much work in positive study: it is a crucial and difficult achievement. Before core problems can be defined with precision, there usually is much prior observation, speculation, debate, and generally diffuse dissatisfaction, a sense of growing mystery rather than illumination. One gradually comes to see through long groping the basic puzzle that a subject presents, and where to begin if a sensible unfolding of theory is to occur. (1980b: 135)

Eckstein's exemplar was Weber's study of the Protestant ethic (1964: 31).

To employ our terms, Eckstein did not understand the context of discovery as an accumulation of ordinary theoretical puzzles or garden-variety real-world problems. He thought that true discovery grapples with *the* principal or fundamental difficulty preventing the growth of knowledge. This seminal problem is deep and far-reaching. It is the central impasse to cumulative theory building. If one is to make progress in inquiry, one must solve this fundamental conundrum with a bold conjecture – for example, that Calvinism caused capitalism.

Among contemporary comparativists, Przeworski, Alvarez, Cheibub, and Limongi (2000) have the clearest claim to having located the core difficulty of a significant research domain. Economic development and political democracy are correlated, not because economic development brings about political democracy, but rather because high levels of economic development immunize democracy against regime change. Pregnant with meaning for a disparate range of observations and ideas, this is no simple empirical finding. Once the core issue – that economic development, political democracy, and regime change are correlated because economic development produces stable democracy – is discovered, others can only reflect back on the history of the field and say, “I wish I had thought of that.” As Eckstein would have told us, years of accumulated observations, speculations, and debates are suddenly demystified. Seminal works, Lipset (1959) for example, are now properly understood. And as happened to Weber's statement of the Protestant ethic, locating the core difficulty focuses but does not end inquiry. Boix (2003), Acemoglu and Robinson (2006), and Epstein et al. (2006) proceed to challenge – but never ignore – the new core understanding.

How can comparativists discover well-motivated questions? Is there a science as well as an art to it? An intraocular test – one that hits you between the eyes – would be invaluable. Herb Simon (Langley, Simon, Bradshaw, and Zytkow 1987) argues that scientific discovery is a form of human problem solving. Given limited cognitive capacities, an exhaustive search of an ill-defined problem in a complex domain is bound to fail – like mime, it will only make the visible invisible. The solution search space must therefore be restricted. Problem solvers thus define a goal and identify path constraints; use simplified schemata to represent the phenomenon and assimilate it to a simple prototype; and employ search heuristics such as pattern matching, decomposition, and localization. Three foci adopted by contemporary comparativists to restrict their problem space are big problems, thorny puzzles, and core difficulties that important explanatory models and foils inadequately address.

SUGGEST AN EXPLANATION

If comparativists can explain problems, if they have a compelling solution, perhaps they do not need paradigms. To deepen variable analysis, contemporary comparativists employ big-concept research agendas, mechanisms, institutions, and middle-range causal arguments.

Explanation = Big Concepts

Social science is a dense network of quantitative data, statistics, and measurements. It is also a rich matrix of qualitative observations, judgments, and cases. And social science is also a chaotic jungle of themes and ideas, hypotheses and theories, perspectives and paradigms. The breadth and depth of social inquiry astonish even its long-term practitioners.

To manage this complexity, comparativists have always turned to concepts as the explanatory bedrock of social science (Weber [1903–1917] 1949; Sartori 1970; Collier and Adcock 1999; and Goertz 2006). By subsuming complex and diverse data under a single category, concepts objectify scientific phenomena and thereby create or construct them: A particular white-collar criminal becomes the scientific problem of white-collar crime. Creating conceptual and operational definitions of vague but commonly used ideas – bureaucracy, public opinion, folk society – is one of the most valuable social scientific methods. Categorical concepts become abstract schemes for ordering literature and guiding inquiry. Like lawyers seeking to define “crime” in all its manifestations, scholars thus develop an “action scheme” as a prelude to the empirical study of “action.” Social kinds, ideal types, and typologies are still other explanatory alternatives rooted in the analysis of concepts (Lichbach 2003: 107–109, 177–180). Conceptual analysis, another method, elucidates the structure of ordinary language and scrutinizes the commonsense usage of words. Philosophical analysis of political doctrines and ideologies, for example a history of the ideas of liberalism, fascism, and communism, is a related approach to concepts.

For example, Weber developed a rich vocabulary, as did Parsons, and Nisbet (1966) tried to capture the set of “unit-ideas of sociology” (e.g., community, authority, the sacred). A more contemporary example is McAdam, Tarrow, and Tilly (2001), who have created a rich, nuanced, and flexible language with which to talk about contentious politics. Such concepts as appropriation, attribution of threat and opportunity, brokerage, category formation, (de)certification, claim making, diffusion, emulation, identity shift, object shift, opportunity/threat spirals, polarization, protest cycle, repression, suddenly imposed grievances, scale shift, trajectories of contention, and trust networks help fashion descriptive and explanatory statements about the dynamics of contention.

Rather than developing a full vocabulary to explain a core difficulty or question, other comparativists, by contrast, offer a single word as a central principle or answer. Gary Wills (1970: 350) makes an interesting observation

about academics: “The more technical, minute, and ‘objective’ a study becomes, the more it stands in need of an embracing, unquestioned, Proposition X: to study in detail the narrow, and be seated on one’s results, one needs a fine acquisition category which, vicariously, to surround one’s subject.” To avoid the inevitable blinders of one’s personal research vision, to overcome a highly specialized and stylized involvement with the empirical and theoretical worlds, and to avoid being seen by one’s critics as pedantic, some comparativists thus employ a single overarching idea, a powerful general concept, as the aegis of their work. Such a grand explanatory concept provides personal and professional meaning – a powerful point of view through which to see the world and a compelling angle of vision with which to understand things in multiple domains. The *grande idée* allows flashes of insight with which to reimagine the world. Such inspired and intuitive leaps can make academic careers and define intellectual legacies.

Tocqueville (1969), for example, placed “equality” at the center of his study. As a generative master concept, it had sufficient height and distance from the many diverse phenomena he studied – social mobility, New England town meetings, party bosses, the frontier, and the Constitution – to reveal and illuminate them. His panoramic view interrelated dozens of seemingly disconnected and contingent phenomena and facilitated the emergence of the whole – *Democracy in America*. His was a theory with a center (Wolin 2001). A century and a half later, Putnam (2000) placed the rise and decline of “social capital” at the core of American life. Section I of *Bowling Alone* defined “social capital,” Section II described it, Section III explained it, Section IV asked the so-what question, and Section V asked the what-is-to-be-done question.

Geertz (1973b: 3) recognized the Tocquevillian approach to thinking and working, noting that many academics search for an idea that is new and different, clever and compelling, and, most importantly, heuristically powerful: “[c]ertain ideas burst forth upon the intellectual landscape with a tremendous force. They resolve so many fundamental problems at once that they seem also to promise that they will resolve all fundamental problems, clarify all obscure issues. Everyone snaps them up as the open sesame of some new positive science, the conceptual center-point around which a comprehensive system of analysis can be built.” Scholarly imaginations have been captured by many sublime concepts that seem to transcend variable-based social science:

Big Concepts: Comparative Politics International Relations

modernity	democratic peace
liberalism	end of history
democracy	new world order
totalitarianism	clash of civilizations
state	a world of tribes
citizenship	global jihad

consociational democracy	American hegemony
justice	globalization
class	borderless world
gender	postcolonial world
ethnicity	endangered planet
relative deprivation	new medievalism
trust	international society
postmaterialism	coming regionalism
American exceptionalism	balance of power
social capital	national interest
civic culture	sovereignty

Each premier idea ties together disparate observations and disconnected themes about structure, culture, and action. By making sense of many seemingly different phenomena, by simultaneously achieving explanatory breadth and philosophical depth – by saying that a hedgehog is also a fox – these central concepts constitute academic gravitas. Catch phrases are today’s social scientific substitute for the grand philosophical systems of the nineteenth century.

If successful, a core motif comes to define a subdiscipline’s research agenda. Bates (cited in Shafer 1994: 4) writes, “When I start a new piece of research, the first thing I ask myself is ‘Who should I take to lunch?’” A subfield’s standards of judgment, taste, and conduct can involve a network – embodied in a language and a community – organized around a core idea. What is a field and who is in it? Why, those who are invited to lunch to discuss its *grand idée*. Members of the research community follow the development of the concept. Researchers market their improvements to this audience. Scientists criticize the agenda and attempt to move the dialogue in new directions. Similar to *The Graduate*, where a well-meaning adult offered the young Dustin Hoffman the key to his future – “plastics” – we mentors tell our graduate students about our “plastics,” for example, social capital. Intellectual establishments, in short, coalesce around ideas, indeed often around a central idea.

After a bit of experimentation, Wittgensteinian wariness and Quinean queasiness set in. The social science game then changes. Participants in the dialogue attempt to limit, specify, focus, and contain the monster. Conceptual analysis dissects folk-wisdom platitudes and explores how the concept works in practice. Conceptual synthesis offers stipulative and regulative norms to resolve indeterminacies, refine meanings, and strengthen applications. As fundamental concepts are explicated and clarified, they are rendered less ambiguous. Given precise content and meaning, they lose their heuristic elan. Geertz (1973: 3) writes that

[a]fter we become familiar with the new idea, however, after it has become part of our general stock of theoretical concepts, our expectations are brought more into balance with its actual uses, and its excessive popularity is ended. A few zealots persist in the old key-to-the-universe view of it; but less driven thinkers settle down after a while to the problems the idea has really generated. They try to apply it and extend it where it applied

and where it is capable of extension; and they desist where it does not apply or cannot be extended. It becomes, if it was, in truth, a seminal idea in the first place, a permanent and enduring part of our intellectual armory. But it no longer has the grandiose, all-promising scope, the infinite versatility of apparent application, it once had.

Geertz seems to have captured the fate of all nodal academic thoughts. While we think and work within the conceptual space opened up by a new idea, a boom-and-bust cycle of core concepts gives comparative politics its faddish quality.

A common path that nodal thoughts travel to oblivion is displacement. After exploring the familiar from a new point of view, the key concept deconstructs. The task of finding a family resemblance gives way to the recognition of an essentially contested concept. When an alternative *grande idée* comes on the scene, there ensues a war of the old and new concepts. Juxtaposing two inter-related master concepts makes both appear ambiguous, multivalent, contradictory, and problematic. Sabetti (2007), for example, shows how studies of civic culture contend with studies of social capital. A second path traveled by nodal thoughts is replacement. Everything about conflict can be explained by relative deprivation. No, everything is better explained by the state. Well, comparativists need to stop and figure out what relative deprivation and the state really mean. Too late. The idea of civil society has taken hold, and no comparativist any longer takes relative deprivation or the state seriously. Hmm, there are those rumblings at Yale and Stanford about bargaining . . .

Two other types of “deep” explanation adopted by comparativists are the *grandes idées du jour*: mechanisms and institutions. With them, social scientific progress is no longer seen as occurring through questioning key concepts. And this constitutes the shift away from a word or phrase to sentences and arguments.

Explanation = Mechanisms

From the perspective of attempting to deepen variable analysis – a possibility inherent in the rich language of variables – the intuition behind mechanisms is straightforward: Comparativists know something works in practice, and we want to see if it also works in theory (Hedström and Swedberg 1998b). We know, for example, that democracies do not fight each other (Russett), high levels of gross national product (GNP) are associated with stable democracies (Lipset), party allegiance everywhere and always correlates with party voting (Converse), and single-member districts with plurality rule support two-party systems (Duverger), but we do not know why this is so or how the connection works. X is correlated with Y, but the correlation needs deconstruction. If asking “why Y” is too broad and open-ended a basis for research, a puzzling empirical correlation provides the classic scientific problem situation. Simmelweis’s studies of childbed fever (Hempel 1966) are motivated by a correlation between the outbreak of disease and physical location in a hospital. Weber ([1904–1905] 1985: 35) begins *The Protestant Ethic and the Spirit of Capitalism* with the observation that “business leaders and owners of capital . . . are overwhelmingly Protestant.”

The goal is then to deepen an X–Y empirical finding, an information-sparse statistical shorthand for relating X to Y, by locating perhaps unobservable causal mechanisms or processes, explanations that provide the details for interpreting or understanding the connection between observables X and Y. Fenno (1986: 6) thus writes: “I do not wish to add to the number of variables we treat; I only wish to argue for the richest possible understanding of some we already recognize as central.” Focusing on a particularly promising pair of concepts might seem to sacrifice breadth for depth, but the particular is frequently the gateway to the general. If comparativists can probe the distance between a dependent and an independent variable, they may be able to identify connections between things that only appear to be separate. The stories behind the GNP–democracy nexus (Boix 2003), the democratic peace theme (Huth and Allee 2002), and the Protestantism–capitalism thesis (Cohen 2002) thus keep growing until they entail global theories of choice, state, society, and international system. When the stories have no other place to go or grow, Geertz’s boom-and-bust cycle kicks in.

Thinking and working in terms of the causal mechanisms behind a single X–Y relationship is an alternative to the positivist reduction of complex concepts to single indicators. Three decades ago, when comparativists were busy correlating one flabby index with another flabby index – for example, the organizational strength of dissident groups with political violence – no one talked about mechanisms (Gurr and Lichbach 1979). Since processes of organization and dissent were compounded into left-hand and right-hand variables, correlations did not need mechanisms to explain them. When our methodological role model shifted from psychology to economics, the crunching of multiple indicators into one index became the crunching of one important measure in a regression equation. If one parameter estimate turns out to be robust between studies – for example, a World Bank GNP variable or a Polity democracy variable influencing civil war – comparativists can ask nonpositivist questions about the causal structures of the world (Lichbach 2003: 173–194; Johnson 2006). Exploring the causal mechanisms behind a core correlation is also an alternative to Laswell and Kaplan’s (1950) proposition inventory and its successor, multiequation models of direct and indirect effects. Since mechanisms and processes were involved in the dozens of left-hand and right-hand variables, large-scale statistical models also did not elicit mechanism talk. Sparse models and austere measurements require the explanatory context provided by causal mechanisms.

What is the source of the mechanisms that could explain a robust X–Y relationship? Aiming to complicate the explanatory account offered by a simple regression equation, $Y = a + bX + e$, that assumes the linear and additive closure of arbitrarily isolated parts, rationalists seek the microfoundations of collective action and collective choice, structuralists the causal complexity of historical dynamics, and culturalists the meaning of political constructions and social facts. Mechanisms thus derive their explanatory power from the theories in which they are embedded:

[A] mechanism **m** is a usually unobservable component of some more encompassing theory **T**. It typically operates at an analytical level below that which **T** seeks to

explain and makes **T** more credible in the sense that **m** renders more fine-grained the explanations that **T** generates. (Johnson 2006: 247; emphasis in the original)

Since a theory's toolbox consists of several mechanisms or models for engineering useful applications, comparativists typically confront long catalogs of mechanisms coming from different types of theories. Moreover, mechanisms derive their explanatory relevance from theories that lead us to believe that the explanatory effects occur only under certain conditions. Mechanisms are therefore always contingent: Given conditions A, B, and C, X connects to Y through mechanism **m**. Exploration of the counterfactual – given conditions A', B', and C', X connects to Y through mechanism **m'** – establishes the applicability of mechanisms **m** and **m'** to real-world situations. These sorts of counterfactuals give comparative inquiry its substantive richness.

Mechanisms also derive their explanatory power from the concrete structures that stand behind correlations, allowing us to address history. For example, Moore (1966) explains his key bivariate hypothesis, “no bourgeoisie, no democracy,” by embedding it in a structural analysis. He specifies how his key variables operate in situationally defined mechanisms and processes, or as Tilly (2006: 422–423) puts it: “Mechanism-process accounts . . . positively welcome history, because their explanatory program couples a search for mechanisms of very general scope with arguments that initial conditions, sequences, and combinations of mechanisms concatenate into processes having explicable but variable overall outcomes.” Moore's historical-comparative analysis thus unearthed the mechanisms that deepened a core if-then hypothesis.

In a theory-then-mechanism approach, grander concepts and theories help comparativists locate critical mechanisms; in a mechanism-then-theory approach, comparativists assemble mechanisms and then create a theory (Lichbach 2005). Both approaches to mechanisms most usefully begin with a correlation in search of causation, a curve in search of theory, a proposition in search of explanation, or a statistical relationship in search of causal enlightenment. While crucial experiments have never ended comparative inquiry, robust and important statistical regularities have become core difficulties that motivated mechanistic explanations.⁴

Explanation = Institutions

Universal patterns of development – democratization, globalization, modernization, industrialization, secularization, and institutionalization – once excited comparativists. Universal patterns of thought and action – rationality, risk aversion, expectations, and Bayesian updating – still excite comparativists.

⁴ Aiming to advance a new class of relational theories over microfoundational (individualistic) arguments, McAdam, Tarrow, and Tilly (2001) advocate a switch from laws to mechanisms. That incomparable comparativist Chuck Tilly (2004: 7–8) effortlessly combines both. Lichbach (forthcoming) explores the reasoning behind their mechanism-based explanations.

However, many of today's comparativists prefer to think about the historically concrete coalitions and cultures that are constitutive of state construction and nation-building (Katznelson and Weingast 2005a; Shapiro, Skowronek, and Galvin 2006). Reacting to a reductive rationalism, Mark Bevir (2007: 293–294) notes how institutionalists sought to preserve such middle-range theorizing “that concentrated on describing broad institutional and behavioral patterns and producing typologies and correlations between social categories . . . the new institutionalism consists of a diverse cluster of attempts to preserve midlevel analysis by emphasizing our social embeddedness and thereby the role of institutional structures and cultural norms as determinants of social life . . . that, in their view, largely settle what happens at the micro level.” Greif's (2006) recent book is a landmark attempt, from the rational choice side, to develop a transparadigmatic theory of institutions that can capture comparative and historical phenomena. I draw on his work to show how comparativists think and work in terms of institutions.

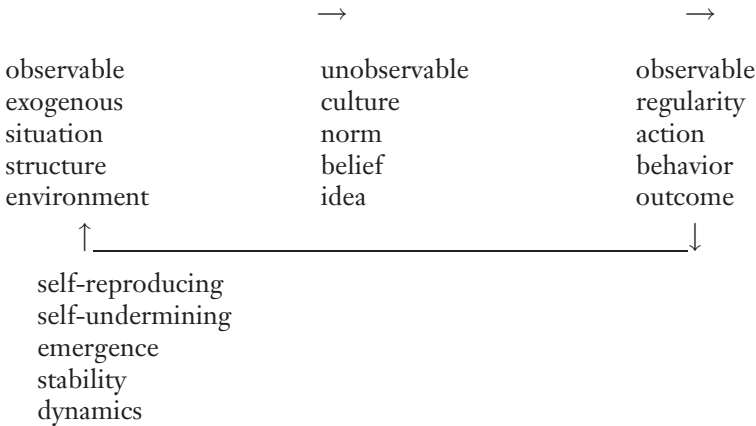
Institutions are governing relationships among optimizing agents. State and market institutions, for example, produce the welfare-related outcomes of peace, stability, and prosperity. Institutions affecting such results can be studied in three ways.

From a system point of view: Institutions are sets of interrelated elements, exogenous to individuals, consisting of observable rules and organizations, and of unobservable norms and beliefs, that generate robust regularities of social behavior.

From an individual point of view: Where more than one behavior is possible or when choice variables are involved, norms and beliefs enable and motivate behavior, and rules and organizations guide behavior, via a self-enforcing mechanism that produces an endogenous equilibrium; this balance of social forces creates and constitutes the institution.

From a transactional/relational point of view: In recurrent social situations, transactions occur in which entities (e.g., social attitudes, opinions, emotions, information) move from one individual who occupies one social position to another individual who occupies another social position; when organizations forge intertransactional linkages between central (e.g., economic) and auxiliary (e.g., political) transactions, institutions arise.

Underlying these three points of view is the economist's favored comparative static exercise:



In other words, exogenous variables determine a Nash (self-enforcing) equilibrium of agents' strategic actions. Institutionalists then ask where this equilibrium of forces came from, what sustains it, and where it can head.

Greif suggests that social scientists identify an institution, or study a governance system, by examining its central and auxiliary transactions, clarifying its origins, and understanding its persistence and change. His approach is to determine the admissibility of a context-specific conjecture about an institution: For such-and-such reasons, in some time and place, a particular institution prevailed and another institution did not. Greif suggests that institutionalists embarking on such an inquiry consider a welfare-related behavioral regularity – the attempt to employ agents, establish large-scale organizations, lend money within a legal system, or acquire institutional resources – that leads to an important outcome such as resource mobilization, political stability, social order, the rule of law, secure property rights, economic growth, a particular income distribution, or a certain distribution of power. Researchers should then identify the relevant self-enforcing institutions – the observable rules and organizations and the unobservable norms and beliefs – that produce observable regularities and affect welfare-relevant outcomes. Since individuals must be motivated to act and since transactions must be linked to endure, the questions are thus: What central and auxiliary transactions are or are not linked by what institutional elements? How and why do these linkages depend on exogenous environmental features?

Greif's rich philosophy of inquiry – his methodology, epistemology, and ontology – are purposefully transparadigmatic. Institutions broadly defined can bridge it all. Institutions can link inputs to the withinputs and the outputs of political systems. Institutions can bridge the structure of the situation and the action of the participants. Institutions can bridge the stability of social arrangements and their origins and transformation. Institutions can combine a microfocus on the individual-level mechanisms behind the origins of preferences and beliefs; a mesostudy of the political struggles over rules and laws; and a macrofocus on the world-historical forces that foster peace and prosperity. Institutions can therefore surely bridge rationality, culture, and structure. If institutions can explain concrete historical and comparative worlds in such profound ways, why should comparativists fight a battle of the paradigms?

Explanation = Middle-Range Causal Arguments

If mechanisms and institutions are the explanatory *idées du jour*, there is an old favorite that has become so much a part of contemporary comparative politics that we rarely give it a second thought. Indeed, this venerable approach to explanation underlies most of the work of most comparativists most of the time.

Robert Merton's (1968) middle-range theories are well-articulated causal arguments. Circumscribed by a research domain, particularized by scope conditions, limited to a set of cases – in other words, relevant here but not there – the reasoning behind middle-range theories is cogent and delimited. Merton thus recommends turning a general problem into a specific one. Universal and generic – transhistorical and transspatial – generalizations are inevitably overgeneralizations. Comparativists should study military coups and not all contentious politics, revolutions and not all social and political conflicts. Middle-range arguments are historically and spatially concrete – for example, contextualizing the preferences of key actors and specifying the origins and consequences of important institutional configurations. After differentiating a general universe of possible phenomena into well-defined subclasses, comparativists should single out a particular subclass for close scrutiny. A triad of assumptions-reasons-deductions works best in a well-specified domain.

Since assumptions, reasons, and deductions can become more transparent when expressed mathematically, Lave and March's (1975) preferred strategy for expressing middle-range theories relies on formal or mathematical models – for example, market and game-theoretic models. The idea here is that social scientists work in modeling traditions and hence need not be the first to model a problem in a particular way. Comparativists can thus begin with a known differential-equation model and produce a new application. The strategy is to challenge an assumption: Is it plausible for this domain? Why should it be made here? The tactic is then to change, modify, drop, strengthen, weaken, complicate, particularize, generalize, or otherwise elaborate the assumption. Once the logic is clear, modelers can extend the reasoning and develop a family of related models within the domain.

Contemporary economic reasoning has become synonymous with this style of applied mathematical modeling (Solow 2005). Economists deliberately simplify real-world detail and complexity, focus on one or two causal or conditioning factors, and exclude everything else. Economists then attempt to understand how this limited number of variables – this representation of reality – works; they follow where the simplification leads and explore its consequences. Since alternative unravelments of reality are always possible, and indeed are encouraged, applied modeling is never automatic. Requiring human judgment, economics is now a technical art like graphic design.

Modernists created idealized abstractions (e.g., general equilibrium models) and positivists developed concrete measurements (e.g., a system of national accounting). However, the combination, even in economics, produced few robust theoretical generalizations with predictive power. Applied modeling of concrete middle-range situations became the solution (i.e., the science). Those who are pushing comparative politics toward this sort of tool-based, engineering-like

strategy of argumentation implicitly acknowledge that there are few general claims, laws, or theories, “only models of concrete, but not necessarily real, cases” (Morgan 2003: 299). Rather than rendering arguments more transparent, however, mathematics can obscure explanations of concrete phenomenon. Wondering whether the loss of falsifiable empirical content in Greif’s catchall theory of institutions is hidden by a mathematically dense game theory apparatus, Clark (2007: 738) asks, “So what insights have we gained from page after page of elaboration on the idea of equilibria and the elements that enter into them . . . ?” Comparativists must separate hypotheses from different analytical sources, disentangle conjecture and correlation, and grapple with causality, essential tasks that game-theoretic models can shroud.

If Merton’s middle-range theories are too vague, and if Lave and March’s applied mathematical models are too formalistic, Eckstein (1980b), once again, struck a compromise that works for many comparativists. An explanation sketch is a bare-bones argument, consisting of perhaps a dozen crisp sentences, that moves abstractly from assumptions in a domain to distinctions, deductions, and conclusions. More than a set of variables or propositions but less than an operational algorithm or schema for a mechanism (Lichbach forthcoming), an explanation sketch is often what we seek from job candidates – a well-crafted PowerPoint presentation that walks us through the core ideas.

Consider Weinstein’s (2007: 12, Fig. 0.1) explanation sketch. Concerned with the micropolitics or internal dynamics of violence during civil war, he focuses on the character and level of violence produced by nonstate actors against civilians. Weinstein assumes that rebel organizations face an initial resource constraint that shapes their ability to capitalize on grievances and opportunities. Analogizing from studies of the resource curse and state predation, Weinstein argues as follows:

1. Since environments rich in exogenous resources – for example, those with access to drugs or foreign patrons – allow rebels to finance warfare, the presence of rich resources leads nonstate actors to exploit the populations under their control.
2. Where rebels lack exogenous resources, they must finance warfare by drawing on the populations under their control. Rebel leaders thus bargain over taxes and spending. In their proto state-building attempts to repress and accommodate their constituents, rebel leaders create credible commitment to ideologically and ethnically mobilizable communities, extracting only the resources necessary for sustaining the movement and providing public goods (e.g., security, market relations, public health, and education).
3. Two types of insurgencies can thus occur: Environments rich in exogenous resources produce opportunistic insurgencies, and resource-poor environments produce activist insurgencies.

Weinstein’s dependent variables thus relate to rebel institutional choice. The organizational forms, practices, and strategies of the rebels involve contractual relations within the rebel group and between the rebels and their potential constituents.

4. *Membership recruitment.* In environments rich in exogenous resources, rebel organizations inevitably recruit thugs who pursue their own short-term material benefits. In resource-poor environments, rebel organizations tend to recruit revolutionaries who follow social norms and invest in their future.
5. *Organizational discipline.* In environments rich in exogenous resources, leaders attempt to coerce an inherently undisciplined membership. In resource-poor environments, leaders share power with a disciplined cadre.
6. *Community governance.* In environments rich in exogenous resources, authoritarian rebel organizations establish predatory administrative structures that allow leaders to extort resources from their constituents. In resource-poor environments, rebel organizations bargain with civil societies under their influence to extract resources; hence, they invest in the mobilization of social endowments and create incentives to secure the consent of the governed; ultimately, they transform local structures of government, employing participatory structures to mobilize constituents for political change.
7. *Strategies of violence.* In environments rich in exogenous resources, rebel organizations abuse civilian noncombatants; brutality and atrocities, theft and looting, abduction and rape are common; victims are killed randomly and indiscriminately. In resource-poor environments, rebel organizations display restraint, discipline, and control; a limited number of noncombatants are selectively killed.
8. *Resilience to challenges to membership.* In environments rich in exogenous resources, coercive tactics persist. In resource-poor environments, disciplined behavior persists but is vulnerable to resource shocks.

In developing his argument, Weinstein assumes that rebel organizations grapple with adverse selection (hidden characteristics of potential members), moral hazard (hidden action of recruits), and credible commitment (trust between rebel organizations and their constituents). However, an explanation sketch provides only what could become an extended model. Concentrating on the observables, Weinstein knows that paradigm-savvy readers can fill in well-known mechanisms. In a question-and-answer session, Weinstein would probably be more concerned with embedding his robust middle-range argument in a larger explanatory context. To establish causal depth – temporal and spatial distance to avoid tautology, banality, and short-run conjunctural arguments about the concatenation of mechanisms – Weinstein could seek the source of a country's resources in the timing of its industrialization, the pattern of mass politics, the forms of state-building, the occurrence of war and trade, and ancien régime legacies. Comparativists generally find that providing a macrofoundation for a middle-range argument garners more interest than itemizing micromechanisms.

Discovery – raising questions, puzzles, and problems – is not enough. Comparativists, after all, are social scientists and not philosophers. Questions, puzzles, and problems must be followed by answers, solutions, and results. These go by many names: Mechanisms and institutions can refer directly to things in the real world; concepts, middle-range theories, mathematical models,

and explanation sketches are often more abstract social-scientific inventions. All deepen variable analysis in ways that avoid a battle of the paradigms and address specific structure-culture-action problems. Most important, all aim to avoid the Grand Theory, Facile Synthesis, and Deceptive System of Thought found in Habermas, Bordieu, and Foucault but profoundly distrusted by today's comparativists (Berg 1998).

PROVIDE SOME EVIDENCE

"Speak (listen) to the Earth, and it shall teach thee" (Job 12:8). Everyone appreciates concrete cases. Even mathematicians will begin with a specific example or distinctive counterexample. Comparative politics, like political science more generally, is an empirically oriented field of inquiry. As members of a field defined by its method, comparativists search for surprising similarities or defining differences among cases. Most importantly, we aim to find out something that we do not already know. Forty years ago, Eckstein (1966) and Lijphart (1975) found Norway and the Netherlands to be *terra incognita*. Today's comparativists still seek new findings. Is China's emerging regulatory state closer to the Anglo-American independent regulatory system or to the Japanese developmental model (Pearson 2005)? The information needed to address this question can only come from spending time with archives, interviews, and first-hand observation. Kalyvas (2006) recounts how his fieldwork on civil war, the site of which was determined by the availability of an untapped archive (247), reshaped his initial understanding of the problem situation (14–15). Tsai's (2007) extensive fieldwork in China shed light on the informal institutions of the private sector and thereby exposed popular myths about China's democratic capitalists.

The history of the *American Political Science Review* (APSR issue of November 2006) further reveals our empiricist leanings: Four of its 10 most frequently cited articles are cross-national aggregate statistical analyses. Today's comparative studies of conflict can be traced to Lipset (1959), who showed how economic development and political legitimacy related to stable democracy, and to Deutsch (1961), who proposed indices for social mobilization and political development. Similarly, today's comparative studies of political economy can be traced to Hibbs (1977), who connected leftist political parties to macroeconomic policy outcomes, and to Cameron (1978), who showed that trade dependence influenced the expansion of the public economy. More recently, Fearon and Laitin's (2003) findings, for example relating the onset of civil war to GNP per capita, have generated a small cottage industry.

Our seminal pieces have thus provided fresh evidence about significant problems. Employing mainstream causal analysis, they divided a parsimonious set of independent variables into plausible rival hypotheses. Nomothetic or lawlike regularities, immutable across a set of cases that were diverse in space and time, were then advanced. These APSR comparativists thus studied how a set of X variables, with a constant set of weights, condition Y variables. Proponents of paradigms, of course, operate with a more complex etiology.

Rational choice institutionalists stress reciprocal strategic interactions and chains of choices in game trees. Historical institutionalists accent path dependence, the unfolding of outcomes or events over time in particular contexts, and endogenous funnels of causality. Constructivist institutionalists emphasize how categories and boundaries of thought create signification and hence construct meaningful social action.

To unite comparativists who favor these different etiologies, the *APSR* comparativists, like all great comparativists, tied theoretical passion to the disciplinary craft of empirical methodology (Munck and Snyder 2007). Hoping to buttress insights with evidence and support theories with research design, they sought findings that were so transparadigmatically compelling that particular ideas found in particular paradigms could become more than “sand castles” (Geddes 2003). Since “theory is cheap, and data are expensive” (Solow 2005: 99), social scientists more generally attempt to improve the ability of quantitative evidence to appraise different sorts of theoretical claims. Drawing upon fundamental work in mathematical statistics and experimental design, economists develop econometrics and psychologists psychometrics. While political scientists have usually borrowed quantitative tools, they have recently begun to contribute their own original methods and techniques (King 1989). Quantitative approaches have thus become firmly established in our discipline. Dynamic models, contextual analyses, measurement approaches, experimental designs, quasi-experiments, and survey methodologies fill our scope and methods courses.

Given our interest in contextual and idiosyncratic factors, it is not surprising that some of our most distinctive contributions to the use of evidence have involved qualitative forms of reasoning. Again not surprisingly, most of this work has concentrated in comparative politics and international relations. Eckstein (1975) and George (1979) made early efforts to elaborate the methodology of case studies, and Lijphart (1971) and Przeworski and Teune (1970) offered seminal attempts to refine the comparative method. More recently, there has been an explosion of interest among social scientists in developing tools for qualitative data analysis. Tetlock and Belkin (1996) explore counterfactual reasoning, Abell (1987) refines the idea of narratives, Ragin (1987) suggests a Boolean approach to qualitative comparisons, Bates et al. (1998a) develop the idea of analytic narratives, Kalberg (1994) codifies Weber’s ideal-type methodology, and Skocpol (1984) suggests a Millsian quasi-experimental approach to comparison.

The most widely discussed analysis of qualitative methodology is Gary King, Robert O. Keohane, and Sidney Verba’s (KKV’s) *Designing Social Inquiry: Scientific Inference in Qualitative Research* (1994). KKV argue that if some basic strategies of research design are adopted, qualitative evidence can have a significant bearing on causal accounts. The book’s success can be attributed to its neopositivist confidence, shared by the many scientifically oriented comparativists who have been perfecting qualitative and quantitative techniques, that evidence can be decisive for resolving theoretical

controversies. KKV emphasize the existence of a single scientific method that joins empirical regularities and statistical replications to causal inference. To provide reassurance against various philosophies of science that doubt the efficacy of evidence for arbitrating among alternative and/or incommensurable paradigms, KKV suggest that scientists search for the observable implications of competing theories and that they locate the falsifiable hypotheses behind warring value commitments. In other words, KKV offer an important rejoinder to culturalists who have flirted with postmodern relativisms, to structuralists who have found a haven in the now dominant realist science theory, and even to those rationalists (e.g., Hausman 1992) in the economics literature who have expressed concerns about the evidentiary basis of economic theory.

Linking econometrics and comparative politics by thinking through seemingly technical assumptions about error terms, KKV also revitalized the field of cross-national statistical analysis. Their book foreshadowed a new style of working with causality. As Rodden (this volume) indicates, nowadays “empirical studies are able to move beyond the blunt cross-national correlations and hone in on the causal mechanisms they imply.” Rodden details how dissecting and discerning cross-national causal linkages involving institutional endogeneity have led comparativists to analytical history. The interesting and exciting puzzles that arise demand a creative mixture of qualitative and quantitative reasoning, of theoretical abstraction and empirical concreteness, and of microfoundational analysis and historical and contextual holism. Comparativists who do cross-national statistical analyses now think and work with countries as more than points and lines on scatter diagrams. As they subject both on-the-line and off-the-line cases to careful theoretical and empirical scrutiny, they engage their theories and cases more fully than their predecessors ever did.

Other recent methodologies, cataloged in Brady and Collier (2004), George and Bennett (2005), and Gerring (2007), further arbitrate the quantitative–qualitative divide. While these methods offer strategies for deepening variable analysis, they aim to not succumb to a battle of the paradigms. I explore three approaches – stylized facts, designs for establishing causality, and analytical narratives – that are among the most popular today.

Evidence = Stylized Facts

Recall that the search for mechanisms can begin by describing reality. Comparativists who employ mechanisms as outlined above typically adopt an extensional orientation and inductive methodology that privileges concrete observation. Research thus begins by knowing something already, indeed something known with certainty about the world. Comparison thus opens not with a theory, law, or method but with a part of empirical reality to be explained. For example, rather than challenging his students to propose an X–Y covering law, Stinchcombe (1968: 13) challenges them to think in terms of an X–Y

observation that can be explained by at least three different theories. This X–Y relationship represents a puzzle about data, a known statistical fact, and an observed regularity about the real world that can serve as a seed statement for further inquiry. To locate mechanisms and processes, one then asks about the who, what, where, when, how, and why of the X–Y relationship. The researcher, that is, explores the observable implications of each theory.

Here, then, is the idea behind stylized facts: If the search for mechanisms is the attempt to deepen a single X–Y relationship, why not begin research by assembling several X–Y relationships? The search for many interrelated bits of information can turn up interesting results: The X–Y relationship works here but not there; the correlation appears under condition A but not under condition B; everyone thought the curve was always positive, but here it is positive and there it is negative; in most times and places there is an X–Y connection, but in certain times and places Z–Y appears; here X develops into Y, while over there Y develops into X. In other words, we do not have just one X–Y correlation; rather, we have something much more complex – and interesting! Our job as comparativists is to assemble these facts and locate the relevant knots of findings. Trusting in the consilience of our inductions, we aim to uncover regularities and establish patterns. This colligation of measurements and assessments, wrenched from various contexts, constitutes our stylized facts. It contains the empirical referents that constrain any argument, explanation, or model that comparativists may wish to advance. Now the detective work begins. We must turn the bits and pieces of experience into clues leading to a *particular* story.

Of course, not all observations offer helpful clues. Instead of assembling obvious observations, scientists focus on the anomalous and the deviant: Something does not work the way theory anticipated; the information does not fit a preconceived convention; and the findings are unexpected and unanticipated. For example, nowadays no one goes around dropping objects – pens, papers, bowling balls, shovels – to assemble challenges to the laws of gravity. Acting strategically to take advantage of serendipity, scientists seek unexpected facts that hold multiple implications.

Researchers then ask: How are these observable patterns and regularities created and sustained, modified and disrupted? What causal processes could result in these data outcomes? Fiorina (1975: 146) writes: “Many, many models will imply X; some fewer will imply both X and Y. Fewer still will imply X, Y, and Z and so forth. The more we know, the more restrictions we can place on our models, and the less likely all of our models have serious misrepresentations of the empirical world.” Popper (1965: 241) would say that Fiorina is clarifying the nature of a problem situation: “What is the general problem situation in which the scientist finds himself? He has before him a scientific problem: he wants to find a new theory capable of explaining certain empirical facts: facts which the earlier theories successfully explained; others which they could not explain; and some by which they were eventually falsified.” Perhaps our bits and pieces of information, all the stylized facts, fit into a larger framework at the core of which is a driving X–Y correlation. We thus look for the data-generating processes that

embed X and Y in a larger framework of causation, processes that turn an X–Y relationship into a more interesting explanandum.

The study of the evidence places the comparativist in the middle of things. We can adopt something like Pierce’s widely cited (e.g., Goldstein 1986: 93) strategy of abduction that “consists in examining a mass of facts and in allowing these facts to suggest a theory. In this way we gain new ideas.” Comparativists can appreciate how an inference to the best available theory or explanation, given all the available evidence, consists of moving inductively from the particular to the general while simultaneously moving deductively from the general to the particular. Comparativists can also see how this double movement sometimes combines the hypothetico-deductive method – the hypothesis plus the background information imply the data – with the bootstrapping method – the background information plus the data imply the hypothesis. Bates (2007: 173) grasps that fieldwork cultivates this sort of inductive/deductive “apprehension”: The social intelligence, practical judgment, and political insight that “is formed through experience, mobilizes intuition, and yields insights that lay the foundations for causal arguments.”

We should not be too positivistic here, however: Stylized facts, not dataset observations, are the explananda of our models. When we observe data via observational procedures, our information is peculiar to our research arrangements, and we always wonder whether we have an artifact idiosyncratic to our particular data-gathering procedures. Comparativists are therefore interested in predicting and explaining something more stable, robust, repeatable, and general than what they measure. Bogen and Woodward (1988: 322) write that “typically scientific theories are expected to provide (what we shall call) systematic explanations of facts about phenomena rather than facts about data.” Hence (351), “empirical adequacy, as we understand it, means that a theory must ‘save’ or ‘be adequate to’ the phenomena, which for the most part are not observed, rather than the data which are observed. [If one] requires that theories save or be adequate to what can be observed . . . this is tantamount to requiring that theory must save the data” that could have problems of validity and reliability.

To work with stylized facts about phenomena, comparativists must operate in a concrete domain that has already established a number of statistical correlations; a subdiscipline where various accepted hypotheses have been advanced; a research program that is rich in systematically recorded comparative and historical evidence; and a problem domain that has accumulated a mass of empirical materials. The domain’s methodologists thus try to invent new data-creation techniques – think of the rise of survey research, content analysis, or social network studies – that enrich the scientific process by elaborating and constraining theories.

Examples of scientific detective work include Simmelweis’s studies of child-bed fever (Hempel 1966) and Durkheim’s (1951) work on suicide. Groffman’s (2001) examples of puzzle solving in comparative politics often begin with stylized facts. Concerned about “the correlates of democracy in the data [that are] potentially informative about the type of models we should develop,”

Acemoglu and Robinson (2006: 48) begin by asking, “What do we know about democracy?” This way of thinking and working is endorsed by statisticians (for other examples, see Freedman 1991 and 1999). It is quite famous in economics. Whereas Milton Friedman’s killer findings about the money supply drove rational expectations theorists, Simon Kuznets’s killer findings about economic growth drove Solow’s models. Kaldor’s summary of known stylized facts and existing knots of findings shows how statistical generalities and empirical regularities guide research in economics (Summers 1991).

Evidence = Designs for Establishing Causality

After Isaac Newton, science took a causal turn. Immanuel Kant was fascinated: “How one thing should arise out of another, when it is not connected with it, according to the law of identity, this is a thing which I should much like to have explained” (cited in Dewey 1973: 14). While they are pursuing stylized facts, comparativists are also aiming to discern *vera causa*, the true groundspring of things. Surely this cuts through the empirical tangle of variables and avoids the theoretical relativism of research paradigms. If comparativists could find out how the world really works, they would acquire profoundly practical knowledge. For example, once comparativists discover that effective democracy, capable states, economic reform policies, and economic development are correlated, they turn to questions of causality. Some seek the microfoundations of rational action, using the rubric of “mechanisms.” Others search for the mesofoundations of the relational embedding of actors, employing the idea of “institutions.” Still others explore causal pathways, offering “middle-range causal arguments.” In short, causally inclined comparativists zoom in and out, use microscopes and telescopes, to discern the whys and hows of correlated worlds. There’s no doubt about it: Today’s comparativists are causal thinkers. A few decades ago, to tell a comparativist that something was “inadequately theorized” or “inadequately problematized” meant that his or her ideas were not embedded in grand paradigmatic traditions and fundamental ontological debates. Today one points to an “inadequate causal explanation.”

“If the field has nothing to offer but weak tests of expansive propositions,” Jonathan Rodden (this volume) notes, “it will quickly become quaint and obsolete.” Rodden thus sees “a shift of attention to matters of causality in comparative politics.” These “efforts to sort out causality” and “get to the bottom of things” begin with robust stylized facts that are profoundly interesting. An empirical result that withstands sustained attempts, using different methodologies, to make it disappear is “believable.” It offers a “secure empirical premise” on which to build. It is Eckstein’s “core difficulty” that must be resolved before progress can be made. Once scholars can “assume that [they] are on to something,” creative theorists set about “unearthing the causal mechanisms” and searching for the plausible causal pathways and processes that connect independent and dependent variables. They can then derive the observable implications of each mechanism and sort out causality. Philosophers of science

call this style of reasoning “statistical relevance explanations.” Salmon (1984: 260–261) thus suggests that “scientific explanation appears to be a two-tiered affair.” Once they have screened out false connections, scientists suggest causal mechanisms to account for the causally relevant correlations.

The basic empirical footing of causality (Stinchcombe 2005) is that a causal relationship can be represented as

$$\Delta X \rightarrow \Delta Y.$$

A difference (change) in one social condition produces a difference (change) in another social condition. If someone is black rather than white, he or she votes Democratic rather than Republican. To evaluate this claim, we need causal information on four observations: two differences/distances on two units of analysis. Causal processes, statistically speaking, thus turn differences/distances in causes (factuals and counterfactuals – the literary theorists were right about the importance of fictional worlds) into differences/distances in effects (real consequences, not artifacts). Causality thus means that differences make a difference or that distances create distance. For multiple units of analysis, causal effects are denoted by differences/distances in the means of treatment and control groups. Speaking probabilistically, causality means that differences/distances alter the probability distribution of outcomes.

Contemporary comparativists employ sophisticated statistical modeling – including sample selection, multilevel duration, and time-series models – that aims to estimate causal effects (Przeworski 2007). After a lull during the 1980s, the profusion of country-level datasets has revived large-N studies and made them central to the field. As indicated earlier, Przeworski, Alvarez, Cheibub, and Limongi (2000) and Boix (2003) employ state-of-the-art statistical methodologies to revisit Lipset’s old economic development–political democracy question. Davenport’s (2007) statistical models show that the domestic democratic peace is not bullet-proof: When facing political dissent, democracies can be coercive and repressive.

Several quantitative and qualitative observational strategies have been developed to improve causal inferences (KKV 1994; George and Bennett 2005; Brady and Collier 2004). These research designs refine theories by increasing their empirical content. Theorists can thus invent new hypotheses that have observational power for the theory at hand, and empiricists can check the robustness of their conclusions by focusing on the multiple outcroppings of their theories. Researchers who triangulate with multiple modes/kinds of observation (quantitative and qualitative, natural experiments and case studies, and so on) exploit the basic idea behind the theory (T)–hypothesis (H)–observation (O) connection:

$$T \rightarrow H \rightarrow O.$$

While this protocol can be used for a single data set, employing multiple datasets allows comparativists to vary the conditions or contexts used in analyzing a model or argument.

Carefully designed fieldwork, involving multiple sources, can produce these datasets. Interested in a number of variables relevant to the Greek civil war of the late 1940s, Kalyvas (2006: 247) used “archives; interviews; and published and unpublished memoirs, autobiographies, and local histories” to create an elaborate coding protocol for collecting data on Greek villages (Table B.3). Selecting one region, he painstakingly “reconstructed the process of civil war in each village” (247). He did the same thing, but in somewhat less depth, in another region. Finally, he supplemented these datasets with a village-level dataset of the entire country. Like Wilkinson (2004) and Posner (2005), Kalyvas is part of a trend toward “carefully crafted research designs implemented at the microlevel to further our general theoretical and empirical knowledge” (248). Country-level fieldworkers, no less than cross-national aggregate-data comparativists, can leverage the multiple observable implications of their theories into causal inferences.

Larry Summers (1991) calls this style of econometrics “pragmatic empirical work.” He offers the examples of Solow on growth, Tobin on Keynesian economics, Friedman on consumption, Philips on inflation/unemployment, Fama on speculative prices, and Moussal on floating exchange rates. Heckman (2000: 89) stresses the theoretical and empirical reach of this style of econometrics: “The best empirical work in economics uses economic theory as a framework for integrating all of the available evidence, tacit and algorithmic, to tell a convincing story.” Achen (2002b) suggests that statisticians work with no more than three explanatory variables at a time and focus on mining the many empirical consequences for the dataset at hand. Moving beyond textbook econometric model building that derives from comparative static exercises on formal micromodels, Summers, Heckman, and Achen recognize that data analysis must be embedded in a larger story. When econometrics moves beyond KKV’s positivism, it moves toward the explanatory mechanisms and theoretical paradigms that can structure thought (Johnson 2006).

Research design, like stylized facts, thus eventually places comparativists in the middle of things. If

$$\begin{aligned} \text{Causal Claims} &= \text{Causally Relevant Correlations} \\ &+ \text{Causally Relevant Mechanisms} \end{aligned}$$

a definitive set of statistical facts and causal mechanisms must be abstracted from the maelstrom of inquiry. Treisman’s (2007: 274) experience councils caution: “It is hard to reach any general conclusions about whether political – or administrative, or fiscal – decentralization will improve or impair the quality of government and economic performance. They will have many effects, driving in different directions on different dimensions. These effects depend on numerous conditions, many of which are difficult to disentangle in theory and to identify in practice.” Translation: After 300 pages of mathematical and statistical modeling, I found that positive and negative factors ran amok. Ouch.

When asked what he thought of Western civilization, Gandhi replied that it would be a good idea. The causal science of comparative politics is another good idea that will not come easily (Przeworski 2004b, 2007). Comparativists now have

more methods than results (Lichbach 2003: 184–185) and philosophers and social scientists more theories of causality than successful applications (Cartwright 2007).⁵ As Rodden (this volume) states, “the demonstration of causality will remain a Platonic ideal that cannot be achieved in practice. But imperfect efforts to approach it are far better than the alternatives.” Rather than fixating on the statistical justifications of causal claims, perhaps comparativists should follow Summers, Heckman, and Achen and also explore their qualitative underpinnings.

Evidence = Analytic Narratives

A major problem with variable analysis is that “variables” are extracted from their social contexts. Once gender, race, education, income, and democracy – staples of the social scientific lexicon – are removed from the situations that give them meaning, they become reified social facts, nominal analytical conveniences, and abstractions independent of time and place. Comparativists are wary of such “conceptual stretching,” of one-size-fits-all ideas. While variables as forces do not “really” exist, comparativists recognize that concrete synchronic and diachronic structures and processes occur in the world. Real social facts are therefore related to proximate other social facts in specific environments. All concrete events are thus lodged in real-world linkages. When comparativists are asked to create a list of abstract causes of some equally abstract phenomenon, their imaginations range over actual contexts and the set of X variables they dream up inevitably overdetermines the set of Y variables. Analyzing concrete situations in terms of abstract variables holds another major difficulty for comparativists: While individually plausible as “the usual suspects,” when taken collectively the X variables could have little or no coherence and might even be self-contradictory. The parsing of necessary and sufficient causes, proximate and remote causes, primary and secondary causes then must guide research into relevant cases.

The alternative is to keep to a single context with the old-fashioned narrative. Since narrators pick their interconnected facts no less than econometricians choose their interrelated variables, social science narratives are best understood as analytic narratives. The stories do not consist of a naturally occurring, inevitable, and essentialist historical chronology. Analytic narratives situate a particular causal story by recounting the theoretically relevant succession of events – what comes before and what comes after. The tale might involve a plot rich in agency (characters with anticipations, setbacks, travails, tensions) and a story line full of structure (contexts, domains, human nature) that lead to truth and reason and/or to chaos and anarchy. The result can be path dependent or complex and open-ended. Static interactions and conjunctures among mechanisms, dynamic

⁵ While graduate students often frontload their introductions (literature reviews) with all the possible plausible rival explanations, experienced scholars (Bates 2008: 131–136; Kohli 2004: 368–380), who recognize the difficulty of establishing causality, develop their own argument and briefly turn to the usual suspects in a concluding chapter.

sequences among processes, and the interaction of static contexts and dynamic shocks can lead to multiple equilibria, unexpected consequences, historical contingency, multiple causation, and a probabilistic etiology. Analytic narratives can thus express the complex causal dynamics emphasized by Zuckerman (1997): forks, branches, critical junctures, time sequences, rates of change, equifinality, multifinality, feedback loops, tipping points, interaction effects, context dependency, strategic interactions, necessary and sufficient conditions, asymmetric relations, heterogeneous units, and unstable variances.

Comparativists can therefore process trace a causal chain between independent and dependent variables in a contextually rich, structured, and focused, way. The results might even be generalizable to out-of-sample historical episodes. Drawing on a long tradition of comparativists, Bates, Greif, Levi, Rosenthal, and Weingast (1998a) advanced an account of analytic narratives that has influenced many rationalists (Lichbach 2003: 211–213). Posner (2005), Boix (2003), and Lust-Okar (2005), for example, elaborate their arguments with the help of analytic narratives. Bates's (1997a, 2001a, b, 2008) grand narratives of the global political economy of Kenya, coffee, development, and state collapse in Africa draw on fieldwork and models, historical case studies and statistics, stories and formal theory. Here is a Hegelian social science with vigor and rigor.

Although clearly analytical, comparative-historical narratives are typically more cautious. Believing that the devil is to be found in the details of concrete historical processes, comparative historians resist slicing and dicing the world into variables. They often draw on Weber and place the ideal type – an explanatory strategy based on natural or social kinds that deftly combines concepts, mechanisms, and institutions – at the center of analysis (Lichbach 2003). For example, Kohli's (2004) problem situation is the great variation in economic growth among late-late developers. While his favored explanatory models come from statist scholars who emphasize tax collection, a disciplined labor force, and state–private cooperation ensuring investment profitability, his inadequate explanatory foils derive from promarket scholars with a state-versus-market mindset who stress the values of an outward orientation, competitiveness, and a self-limiting, minimalist, laissez-faire state. Ideal types of late-late industrializing states – neopatrimonial (Nigeria), fragmented multiclass (India), and cohesive capitalist (Korea) – are the core of Kohli's analysis. He explores the mechanisms and processes that constitute the institutions of each type of state; seeks the roots or origins of state construction in colonialism, anticolonial nationalist movements, and the coercive politics of national armies; and traces the consequences of each state type for the state's power or capacity – rooted in state ideology, state organization, and state–society links – to promote development. With these rich and detailed comparative analytical histories in place, Kohli (2004: 417) believes that he can perhaps begin to understand the different patterns of industrialization and divergent economic growth rates of late-late developers: "It is difficult to isolate the relative significance of a number of causal variables via comparative analysis of a few cases. Immersion in the details reveals at best a feel for what might be the most significant causal dynamics at work. I claim no more."

If a strictly deductive approach to research begins, as suggested by KKV, with one's pet explanation (a big concept, mechanism, institution, or causal argument) and involves thinking and working in terms of its observable implications, a more inductive approach begins with one's pet case and involves locating causal mechanisms and process within analytic and historical narratives. The approaches can be complements and not substitutes. Riker (1962) and Olson (1965) thought and worked in terms of the observable implications of theories and in terms of the causal processes lodged in analytic narratives – and their books became classics. In search of a compelling causal story, today's comparativists add statistical modeling to the mix.

Kant's mysterious and contested idea of causality is now at the center of comparative politics. Echoing quantitative comparativists who look to stylized facts and research designs for establishing causal claims, qualitative scholars have proposed several approaches to pragmatic empirical work that also place them in the midst of things. In addition to analytic narratives, comparativists have explored process tracing, within-case analysis, causal process observations, historical explanation, and robust processes (Brady and Collier 2004: 93, 252–254, 277–278; George and Bennett 2005: chaps. 7, 10; Gerring 2007: chap. 7).

MULTIPLE PARADIGMS: DEADENING METAPHYSICS OR VITALIZING FRICTIONS?

Those seeking truth typically want comparativists to offer some evidence; those searching for beauty want a clever explanation; and those hoping for justice want to discover real-world problems. With these aims in mind, contemporary comparativists have worked hard to fashion alternatives to paradigm-based inquiry. Rather than having to reinvent local wheels, we possess a set of best – or perhaps least bad – practices that can be transferred from one research context to another. Now take a step back and ask, why is it that so many contemporary comparativists want to avoid thinking and working in terms of paradigms?⁶ Are there reasons to bring paradigms back in?

Deadenning Metaphysics

Some suggest that canonical rational choice theory no longer exists in comparative politics. As its proponents gave up the goal of product differentiation and moved toward the idea that rationalist thought is all of social science, the separate assumptions, boundaries, and identity of the approach broke down (Lichbach 2003). Rational choice theory has been enlarged in at least three ways. The search for microfoundations led to bounded rationality and behavioral economics. Second, transaction cost economics moved beyond the study of

⁶ Lichbach (2003) discusses the pros and cons of a dialogue among multiple paradigms as against the hegemony of a “messy center.”

exchange in efficient markets and now extends to all parts of institutionalized social life. Finally, analytical narratives blend with historical institutionalism and case-oriented methodology, allowing comparativists to study macrostructures – institutional development in global and historical context – using a rich mix of substantive cases and analytical models. Rationality has thus been combined with symbols (Thelen 1999: 377), signals (Bates et al. 1998a), focal points (Chwe 2001), and culture (Fearon and Wendt 2003; Bates et al. 1998a,b; Ferejohn 1991). Rationality has also been combined with structure in the form of rational choice institutionalism (Katznelson and Weingast 2005; Weingast 2002: 687). If rational choice theory searches for mechanisms and not laws (Elster 1989a; Petersen 2001, 13), structure and culture define and elaborate the approach rather than bound it. The initial applications of rational choice theory evidently misunderstood what the approach was about: “[T]here is no dichotomy at all between structural and strategic approaches – they are one and the same” (Acemoglu and Robinson 2006: 86).

Rational choice institutionalists, such as North, Bates, Greif, Levi, Acemoglu and Robinson, and Przeworski et al., now approach questions much like historical institutionalists. Their perspective might as well have been defined by Katznelson (this volume):

Grand ambitions to address big issues and major questions. The idea is to combine political economy and political sociology to comprehend the total organization of society and address the large-scale developments, institutions, and ideas behind states, markets, and communities.

Positive and normative concerns about the performance of regimes during the deeply disturbing twentieth century. The aim is to provide guideposts for action and resources for a descent politics.

Place liberalism at the center of analysis. The contractual approach leads analysts to study liberalism’s character as ideas, institutions, and agency, and to explore its accomplishments and flaws, potentials and vulnerabilities, and deaths and rebirths. The goal is to fashion a more credible liberalism in the face of state and nonstate actors’ endless pursuit of power and predation.

A methodology that moves beyond variable-centered approaches that characterize behavioralism’s hypothesis-testing. Researchers who combine history and microfoundations place institutions and mechanisms at the center of analysis. Strategies of causal inference thus have been the subject of methodological innovations designed to test game tree-like arguments. For rational choice institutionalism, this has meant selection bias; for historical institutionalism, it has meant process tracing and case study methodology.

If every piece of social scientific reality contains an important bit of rationality, is not rational choice theory all of social science (Lichbach 2003)? Like an earlier generation’s experience with statistics, rational choice theory has become part of the metalanguage for social scientific reasoning. Statistics provides the syntax

or grammar for constructing causal statements, rational choice theory the semantics or content of arguments.

Paradigm busters therefore argue that mutual engagement and interaction have produced intellectual cross-fertilization. Sharing theories, blending concerns, and mixing methodologies have yielded collaboration among paradigms. Border crossers, borrowers, and arbitragers have generated convergence in ideas and methods. Overlapping agendas, points of tangency, intellectual intersections, and common ground now characterize comparative politics, as most comparativists recognize that paradigmatic approaches are complements and not substitutes. In the eclectic, sprawling, and diverse literatures that characterize the field, drawing hard-and-fast boundaries between frameworks is fruitless. Because rationality, culture, and structure are porous, they work together rather than constitute separate zones of inquiry. Many therefore think that comparative politics has entered a stage of dialogue and rapprochement, integration and synergism. Since the world is increasingly complex and multifaceted, comparativists employ a wide array of tools to explain outcomes, policies, institutions, behaviors, and values.

Attempting to create an overall ranking of the approaches to the field by weighting the strengths and weaknesses of rationality, culture, and structure is thus a chimerical exercise. Most comparativists stress the contingencies or circumstances that permit each framework to yield its fruits. In making their evaluations with respect to particular applications, scholars need not be partisans of a particular viewpoint. They can be neutral observers who describe, compare, and contrast different sets of explanans. As unofficial umpires or judges, they can establish standards for good and bad arguments. Comparativists in the fields of economic development, state construction, electoral politics, and social movements can thus adjudicate claims and counterclaims, picking and choosing among the best practices of the approaches.

Viewing the work of comparativists in terms of a battle of the paradigms therefore yields a false stereotype or caricature of present-day disputes in comparative politics. There are no natural disagreements, inherent conflicts, enduring cleavages, impassable boundaries, and persisting separations among rationality, culture, and structure. Indeed, the battle of the paradigms itself constructs and reifies divisions among comparativists. If distinctions among the approaches were socially and politically constructed, they can now be deconstructed. The intellectual world is neither polarized nor cooperative; rather, academics choose whether or not to make the creation of social thought a harmonious social endeavor.

Since there are no natural distinctions among paradigms and since the spirit of this academic age involves deconstructing distinctions, those who write about the battle of the paradigms appear to lack depth. To the deconstructionists, paradigm warriors overlook significant interconnections among programs, ignore qualifications to each program, and slight the complexities of application. Each paradigm is presented as an overschematized and poorly summarized system. The alignment within each reified binary, with the interrelationship of

the elements identified, is never fully satisfactory. Idiosyncratic impulses and reasons for scholarly choices are slighted, as the shifting of key terms from context to context is not recognized. Those who write about the battle of the paradigms therefore can never control their language sufficiently to satisfy the deconstructionists. A precise nomenclature that limits distances from scholarly terms to academic research tests escapes them. And without the satisfactory deployment of texts, the deconstructionists maintain, those who advance an image of a battle of the paradigms can never produce a compelling narrative.

In short, many comparativists see paradigms as a hindrance to understanding (Hirschman 1970). Whereas paradigms were once the creative source of problems, arguments, and data that took us beyond behavioralism, they have now become faddish knee-jerk reactions to other faddish knee-jerk reactions. Paradigms simply get in the way of scholarship.

A battle of rationality, culture, and structure is therefore an inferior starting point for comparative inquiry. A focus on research schools diverts attention from better (i.e., more cumulative) approaches to social science. Those who believe that thinking and working in terms of paradigms do not creatively deepen variable analysis propose to reconfigure positivism's variable-based world using the aforementioned approaches to discovery, explanation, and evidence. Comparativists nowadays care about motivation, that is, about the problems and puzzles that undergird their work. They also care about theories – middle-range arguments about, for example, democracy and civil war. And comparativists care about evidence, that is, the substantive data behind analytical claims. These approaches can navigate between the Scylla of deconstructing the world into variables and the Charybdis of reconstructing it into reified paradigms.

Vitalizing Frictions

If this picture of the disciplinary center of comparative politics were true, there would be no need for a second edition of a book devoted to rationality, culture, and structure, unless, of course, a decent funeral has become a valid motivation in the academic publishing world. Yet somehow traditions are sticky, communities enduring, and research path dependent. Recall the quotation from Dryzek, Honig, and Phillips (2006) presented early in this chapter. They suggest that “political theorists do not readily position themselves by reference to three or four dominant schools that define their field” and then attribute this idea to “liberal, critical, and post-structural theorists.” While advancing their claim that there are no research schools, the claimants evidently form research schools. Nice.

Thelen, Laitin, and others are partially correct in noting that the turn by comparativists to institutions has brought two important developments. First, there is greater interaction among paradigms. Juxtaposition of the approaches has led to mutual discussion. Second, the past decade has seen mutual exchange. Paradigm warriors have found common ground – complementarities – among the approaches. They have located a messy center of convergence among the paradigms, for example, with respect to institutions. Laitin's vision of

the field – statistics in the morning, formal theory in the afternoon, and a round of fieldwork in the evening – has indeed attracted talented graduate students. Like our undergraduates who nowadays double and triple major, their spectacular hunger to be in the midst of things leads them to dwell in possibilities (Edmundson 2008).

However, Thelen, Laitin, and others are also partially wrong because the past decade has also sharpened contradictions among the paradigms. Although Fearon and Wendt (2003: 67) maintain that “the idea of a battle royal or ‘Great Debate’ between rationalism and constructivism is appealingly dramatic, but properly understood many of the issues dissolve upon close inspection,” Wedeen (2002: 726) counters that “the gulf between interpretivists and rational choice theorists may be too wide to bridge. In many ways, the concerns of rational choice theorists and interpretivists are simply incommensurable.” Rational choice institutionalism differs from historical institutionalism with respect to the context of discovery (big problems vs. thorny puzzles) and with respect to the specific mechanisms for studying conflict/cooperation and stability/change (Pierson and Skocpol, 2002: esp. 705–707). Moreover, intellectual space is not symmetrical: The distance from rationalism to constructivism is a short car trip, whereas getting from constructivism to rationalism is a sea voyage. Trade-offs among the usual suspects remain: generality versus specificity, problematizing preferences versus assuming them, microfoundations versus holism, functional theories versus historical theories, variables versus mechanisms, and causal versus configurational versus constitutive thinking. And the old saws still hold: strong on rationality, weak on culture and structure; strong on culture, weak on rationality and structure; strong on structure, weak on rationality and culture.

A perusal of Katznelson and Milner (2002b), an APSA-sponsored volume on the state of the discipline, demonstrates that while paradigm wars may have exhausted international relations scholars, for those of us in comparative politics, the battle endures. The volume demonstrates the continuing relevance of the battle of the paradigms and shows how the communities’ different histories, questions, and methods still structure inquiry. For example, Margaret Levi’s (2002: 38) contribution begins by noting that “the most recent research on the state comes from several perspectives.” Other contributors choose sides. Atul Kohli (2002: 86), for example, writes that “while I pay some attention to paradigmatic battles, the focus below is on research and debates within a shared intellectual tradition.” Pierson and Skocpol (2002: 695) write: “What historical institutionalists broadly share becomes apparent when we juxtapose their ways of asking questions and seeking answers – their research strategy – to the strategies normally used by behavioralists and rational-choice modelers.” Evidently, lines of demarcation remain, differences and disagreements persist, and ideological quarrels continue as scholars choose to sit at different tables.

Practicing comparativists thus discover the necessity of choice. Lieberman’s (2003b) comparison of race, regionalism, and tax policy in Brazil and South Africa is an extremely thoughtful synthesis. He develops a dynamic, path-dependent model that connects the structure of national political communities to the

mobilization of group identities and, eventually, to upper-group political strategies. Drawing on paradigmatic statements of structure, culture, and rationality, Lieberman gives careful consideration to the best ideas from historical institutionalism and rational choice institutionalism. He concludes, “[n]evertheless it is useful to try to be clear about real scholarly disagreement” (21). He then adds, somewhat painfully, that “I do not find the rational choice/game theoretic treatment of politics to provide significant added value for understanding the role of institutions for the problems and cases I consider.” Lieberman’s case studies, although full of talk about class relations, bargaining power, and political strategies, employ neither collective action theory nor the spatial theory of elections. Indeed, they do not draw upon the nuts and bolts of any recognizable rationalist theory. The same holds for Brownlee’s (2007) fine historical and institutional study of authoritarian rule in Egypt, Iran, Malaysia, and the Philippines. Brownlee somehow manages to study elite factionalism, weak regimes, elections, parliaments, and party-based coalitions without recourse to any of the instruments in the rationalist toolbox. Similarly, Solingen (1998) fashions interesting and important arguments about coalitions, entrepreneurs, logrolling, preferences, institutions, and policy outcomes that draw on neither collective action nor collective choice theories.

Those who deny the necessity of choice run into trouble. Suppose, following Greif (2006: 30), that we define an “institution” as “a system of rules, beliefs, norms and organizations that together generate a regularity of social behavior,” thereby encompassing all formal and informal institutions. And suppose, in good rational choice fashion, we suggest that at the individual level, desires + beliefs → actions and that at the collective level, preferences + institutions → outcomes. Our postparadigmatic – that is catch-all – tool will likely invite the question “Is institutional theory all of social science” (Lichbach 2003)? Clark (2007: 736) thus gibes at Greif as follows: “Once we are compelled to admit, however, into the explanatory apparatus almost the entire sociological zoo of ill defined and unmeasured constructs [‘cognitive norms,’ ‘the social and normative foundations of behaviour,’ ‘losses of esteem,’ ‘fairness,’ ‘social exchange’] we lose all explanatory power.”

Interestingly, true-believer paradigm warriors are still around, attempting to push their approach to its limits, using their perspective as a model to play off its foils. To find them, however, one has to move to the margins of comparative politics. Some (Dixit 2004; Enders and Sandler 2006) still write as the early Tullock wrote: as if they were opening up entire fields to rational choice theorizing. While their work, for example, on terrorism and the rule of law, should interest comparativists, their bare-bones models do not engage cases in depth, and they mostly ignore the comparativists who use rational choice theory to write on these subjects. Similarly, some neo-Marxist world-systems sociologists (Silver 2003) continue to produce structural accounts, for example, on workers’ movements and globalization, that should interest comparativists. Most comparativists, however, will find their purist treatment of structural issues strangely outmoded, as these accounts mostly ignore collective action and framing. Their

work recalls an earlier period when comparativists were not yet able to fashion deft combinations of structure and action and tie them to extended analytical narratives.

Culturalism or constructivism is in the forefront of the paradigm wars of the new millennium (Brass 1997; Ross 1997; Wedeen 1999; Schwedler 2006; Blyth 2002; Berman 1998, 2006). We have methods and textbooks (Klotz and Lynch 2007), exhortations and polemics (Norton 2004), and paradigmatic overviews (Green 2002b; Chabal and Daloz 2006). Hard-core constructivists are challenging the core beliefs of rational choice institutionalists and historical institutionalists. Starting with race, class, gender, ethnicity, nationalism, and the state, constructivists have problematized mainstream ideas and destabilized conventional thinking about individuals and groups. Blowing offshore a mere decade ago, constructivist winds are now a gathering storm that is expected to make comparativist landfall during the coming decade. Rational choice culturalism or culture as equilibrium, Laitin's (2007) attempt to extend choice to the selection of identities, will prove insufficiently constructivist for social constructivists. It has already generated harsh words (Laitin and Weingast 2006; Norton 2006). So here is a revitalized *Weltanschauung* that will eventually have to be fully engaged (Fearon and Wendt 2003; Chong 2000; Chwe 2001). Sociologists have already taken up the challenge (Adams, Clemens, and Orloff 2005), and the connections to a social logic of politics (Zuckerman 2005a; Zuckerman, Dasović, and Fitzgerald 2007) have already been drawn (Sabeti 2007).

International relations scholars are already there. A dozen critical evaluations of constructivism in international relations have appeared in the past decade (Adler 1997, 2002; Checkel 1998, 2004; Hopf 1998, 2000; Dessler 1999; Palan 2000; Guzzini 2000; Finnemore and Sikkink 2001; Dessler and Owen 2005; Pouliot 2004, 2007). Where is the parallel interest among comparativists? How could culture be more of a concern in international relations than in comparative politics? How did international relations steal culture from comparative politics, a field that reckons Almond and Verba as one of its classics and Weber as one of its founders? Our unwillingness to take theories and theorists seriously, our rejection of the sorts of foundational debates that occur in international relations, has left us lagging behind in our engagement with new theoretical thinking. If the experience of international relations is any guide, and Kanchan Chandra's chapter in this volume shows that it is, the messy center of comparative politics will benefit from theoretical and empirical encounters with the constructivists, even though it will be unable to absorb its postmodern wing.

Moreover, the past decade in comparative politics corresponds to what I predicted in the first edition of this book and in Lichbach (2003): The interplay between rationality, culture, and structure has encouraged a great deal of critical and creative thinking.

Paradigm thinking has forced comparativists to scrutinize the inadequacies of existing scholarship. Paradigms have led scholars to explore prevailing assumptions, prejudgments, and presuppositions and have thus led them to revise, replace, or firm up their commitments, preferences, and beliefs. For

example, while classical liberalism emphasized private/public boundaries, feminist theory pointed out that the distinction defined away power relations in the domestic sphere. While behavioralism, for another example, argued that observable behavior is crucial to science, structuralists pointed out that nonaction is crucial in power relations. Finally, while the classic theorists of political culture – Gabriel Almond, Sidney Verba, and Ronald Inglehart – simultaneously reified national cultures and reduced them to individual preferences and beliefs, Clifford Geertz understood culture as “publicly shared symbols and the cultural practices that social and political actors deploy in the contested effort to impose conceptual order on otherwise indeterminate experience” (Johnson 2003: 93). While positivists thought that observable relations are the only part of science worth talking about, these examples show that metaphysical issues also bear scientific scrutiny. Investigations of our regulative ideas about rationality, culture, and structure, reflections on rival commitments to practical reason, social construction, and political institutions, do not entail meaningless debates about transcendentals. New paradigmatic perspectives illuminate the conceptual problems of older approaches that hinder scientific progress.

Paradigm thinking has also been creative thinking. The seminal first-generation pieces in rationality, culture, and structure seem to have burst upon the scene with novel ideas about old problems. Bates (1989) brought rationalist political economy to the many-sided study of agrarian development in Kenya. Scott (1985) brought culturalist and constructivist understandings of hegemonic identities and relationships to the complex problem of social order in a Malaysian village. Skocpol (1979) unveiled state theory to clean up the study of revolution in France, Russia, and China. These classics of comparative politics remade the chaotic problem situations from which they emerged.

The wholesale importation of paradigms that rework the mainstream is still occurring. When Hall and Soskice (2001) advanced a new approach to comparative political economy, they drew upon pure theories that were developed outside the discipline. Similar to an earlier generation of comparativists who turned to economics and found public goods theory and the spatial model of voting, Hall and Soskice turned to economics and found the new information economics. Another example: After showing that “economic-structural, political-institutional, ideational, and rational choice theories do not yield fully convincing explanations for the politics of economic adjustment” in the fragile democracies of Argentina, Brazil, Peru, and Venezuela, Weyland (2002: 36) developed a new explanation of market reform that draws on Kahneman and Tversky’s prospect theory. So it goes. Comparativists use ideal-type research programs and paradigms to generate new research ideas that serve as metanarratives to structure the messy empirical world. Pure approaches to theory remain heuristic devices that generate new middle-range applications that sort reality. To study institutional origins, reproduction, and transformation, even historical institutionalists like Thelen (2004) position themselves within rational choice debates about collective action, credible commitments, and path dependence.

Today's comparativists, however, find themselves in the maelstrom of a field defined by Bates, Scott, and Skocpol. They typically begin with a rationally, culturally, or structurally defined problem situation and often try making a creative move by going against the grain. Beginning with the constructivist finding that ethnic identities change from situation to situation, Posner (2005) uses rational choice institutionalist ideas to determine which of a country's many potential ethnic identities and cleavages – race, language, religion, and so on – constitute the basis of political mobilization and public contestation. Varshney (2002) can be thought of as beginning where Posner (2005) leaves off. Rational choice institutionalist approaches assume that political elites try to create winning coalitions in preexisting institutional frameworks of politics, such as federalism, consociational democracy, or single-member electoral districts. Different political institutions lead to different elite strategies that are linked to ethnic war or peace. Studying how preexisting inter- and intragroup communal and associational networks produce institutionalized peace systems or institutionalized riot systems, Varshney locates the mechanisms of local civic life that undergird the country-level correlations between leadership strategies, political institutions, and ethnic conflict. Petersen (2001, 2002) can be thought of as taking off from where Varshney ends. Starting with networked patterns of communal structures – size, homogeneity, centralization, and density – Petersen probes how community-level norms (reciprocity) and individual-level emotions (fear, hatred, resentment, rage) produce ethnic conflict. In sum, the legacy of Bates, Scott, and Skocpol – first-generation rationalists, culturalists, and structuralists – is to be found in paradigmatically defined problem situations that move second-generation students of ethnic politics – Posner, Varshney, and Petersen – along creative cycles of rationality → structure → culture. One therefore expects an ambitious graduate student to begin with Petersen's culture (individual emotions and communal norms) and then make a move to Posnerian rational choice institutionalism.

Creative research thus involves turning an interpretivist study into a positivist study; a data analysis into a narration; a behavioral approach into a cultural one; a methodological individualist approach into a holist one; a realist perspective into a constructivist one; a focus on choice into an analysis of constraints; a study of statics into a stress on dynamics; an analysis of conflict into one that explores consensus; and a study of context into an examination of emergent properties. Or one can go the other way! The grand debates of social theory are powerful heuristic devices that are the engines of progress in comparative politics. Moves against the grain, a switch of positions, advance a field.

Internal developments in a paradigmatically defined research program might unwittingly generate such moves. Consider the institutionalist program that Rodden (this volume) discusses. If rational choice research has shown that democracy matters for prosperity, and if it has also demonstrated that democracy is endogenous, is it really the case that institutions matter? Perhaps the key causal forces that permit autocracies to move to democracy are to be found in the structural conditions of a capitalist economy. Statistical testing aimed at probing causality has thus led cross-national

quantitative scholars to reread Marx and Weber, Moore and Skocpol, and Bendix and Luebbert. As Iversen (2006: 617) discovers, a grand structural-historical turn to questions about the persistence and change of institutions can start the flow of creative juices among those who specialize in microfoundations: “A decade ago nearly all comparative political economists would have called themselves institutionalists. Today an increasing number of scholars are convinced that the only way forward is by going back – back to the origins of institutions and the conditions that gave rise to them.” One suggestion is that prosperity occurs where “a sizeable middle class essentially serves as a buffer against radical demands for redistribution under democracy.” To avoid a straightforward return to Moore’s “no bourgeoisie, no democracy” or to Lipset’s “no modernity, no democracy,” the “new structuralists” employ the economist’s style of applied mathematical modeling – “carefully identified historical constraints with rigorous theorizing” – and show, for example, how the growth of a middle class shapes the incentives to compromise (Iversen 2006: 618).

The great debates in the social sciences about rationality, culture, and structure are thus less about the true or profound organizing principles of the world than they are about the most fruitful heuristics for discovery. As Abbott (2004) shows so well, debates about paradigms raise new questions, open the way to new possibilities, and offer new opportunities. Rather than using paradigms to debunk or demolish hard-working social scientists, creative scholars use paradigms to fashion their own ideas. Creative confrontations among paradigms drive research by stimulating problem formation and advancing problem solution.

Paradigms are therefore particularly fruitful heuristic devices. Creative, imaginative, and even ingenious social science can emerge out of a critique and reconstruction of existing social scientific paradigms. A single paradigm can be used in ways that are novel and original. The juxtaposition of several paradigms, the careful use of models and foils in Platonic dialogues (Lichbach and Seligman 2000), can lead to new ways of looking at things. Two solid rules of inquiry thus exist: To derive new insights, employ internal critiques that exploit the inconsistencies of a favored discourse; to sharpen one’s argument, employ external critiques that bounce ideas off something completely different. Rather than attempting to construct a transcendent vantage point, creative social scientists adopt a playful attitude toward multiple kinds of argumentation.

While research programs are valuable heuristics for explaining experience, they can be used poorly. A very smart graduate student of mine was doing fieldwork in Africa. She e-mailed me this question:

Would it be too jargony for me to say that I am working within a sociological institutionalist framework as opposed to a rationalist institutionalist framework? Does that muddy the waters we are trying to clear?

I replied as follows:

Sociological institutionalism and rational choice institutionalism are heuristic devices that we use to help explain the world. They are highly abstract frameworks and

paradigms that can generate a variety of middle-range explanations and understandings that are the workhorses of social science. So if you begin with sociological institutionalism and rational choice institutionalism, move quickly to the mechanisms and processes that have observable referents in your country case studies.

After fielding a series of questions about the “real” or “important” differences in the approaches, I recognized the underlying concerns and responded as follows:

One way to look at metaframeworks is as heuristic devices to generate competing arguments and ultimately plausible rival midrange hypotheses. The issue then isn’t about who you are, your identity as a social scientist. The frameworks can’t be reified into personal dos and don’ts. Nothing creative comes out of that sort of restricted thinking. Social science is about the ideas you entertain. Another way to look at frameworks is thus as means to develop your own ideas. You creatively bounce one framework off the other, seeing the world from this perspective and then that perspective, all the while looking for interesting and verifiable things to explore that could not have come from any one framework alone. In either way, you need to move from the critique of assumptions to the construction of arguments.

I added that a literature review is not a theory and hence should not be written as an assemblage of dissected variables and broken hypotheses juxtaposed in a single narrative or regression equation. A literature review is a search for paradigms as heuristics to solve problems, a search to discover the key puzzles, mechanisms, and evidence needed to create an original theory, develop a new argument, or advance a different idea. By helping us critique the underlying assumptions of existing studies, paradigms help generate these new perspectives on old research. Novel research moves come from the critical thinking and creative thought inspired by paradigms.

Novel research moves do not result from syntheses of syntheses for a simple reason: A new synthesis is parasitic on an old divergence. Synergisms result from using pure theories as heuristics to unify findings in a research domain. Integrated sets of ideas derive from the basic and fundamental understandings about the world that are found in paradigms. Creativity in comparative politics often involves the intellectual arbitrage that comes from the mutual discussion and the mutual exchange among ideal-type approaches. Competition among paradigms is therefore neither irrelevant nor counterproductive; it is the engine of creativity. Research can progress by a battle of the paradigms.

COMPARATIVE COMPARATIVISTS: PARADIGMATIC AND PRAGMATIC

In Greek political thought, “theory” meant “journey” (Wolin 2001: 5, 34–35). The comparative theorist is a traveler, observing different real-world countries and imagining in the mind’s eye different ideal-world societies. When in doubt, comparativists compare by journeying to different countries and journeying down rationalist, culturalist, and structuralist paths.

Yet, there are alternatives to paradigms. At least three modes of discovery (big problems, thorny puzzles, core difficulties), four types of explanation (big concepts, mechanisms, institutions, middle-range causal arguments), and three forms of evidence (stylized facts, designs for establishing causality, analytical narratives) have attracted the attention of contemporary comparativists. We have tried at least 10 ways to deepen variable analysis and yet avoid bringing paradigms back in. If paradigms divide us, these alternative foundations of inquiry help us all get along. And if these paths toward kumbaya comparative politics fail, we can always choose our heroes carefully: Weber and Tocqueville can unite us, Marx and Foucault divide us.

For all the reasons discussed in Lichbach (2003), using rationality, culture, and structure as models and foils can produce good social science, political science, and comparative politics. Those who think in terms of mechanisms, puzzles, models, institutions, designs, narratives, and so on can also produce creative research. Note the conditionality – the “can” – of this statement. Variable grubbing or paradigm grubbing will not produce good social science, and neither will puzzle grubbing, mechanism grubbing, narrative grubbing, or any other kind of grubbing. Who wants to read about the two dozen mechanisms behind the French Revolution? Who wants to read a score of country-level narrative redescrptions of economic growth and democratization? One is therefore led, as if by an invisible hand, to assemble the building blocks scattered throughout this chapter and to issue a single foundational statement.

Harry Eckstein, wanting to get to the heart of the matter, emphasized the core problem, the decisive explanation sketch, and the crucial case study. Eckstein also believed in asking theoretical questions in ways that promise empirical answers. Social science employs rigorous methods of addressing problems – sticking to operational, testable, and convincing rules for explanation and evidence.

In the social sciences, alas, problems are never core, explanations never decisive, and evidence never crucial – at least not in some final sense. The framework used here suggests a more general algorithm: Discover a difficulty, suggest an explanation, and provide some evidence. Good research involves substantive problems + research hypotheses + empirical methods. Since explanations are continually being articulated and refined, proposed and revised, explanation merges with discovery and testing. Our approaches to discovery, explanation, and evidence thus always operate in the midst of things. One can begin with the problem, continue with the mechanism, and conclude with the design. Or one can go the other way. Two starting points, one inductive the other deductive, are possible:

1. start with differences/distances;
2. speculate about the mechanisms connecting cause to effect;
3. choose units of analysis by developing analogies among the differences/distances; and
4. figure out the contexts in which the theory works.

Or

1. start with the mechanisms connecting cause to effect;
2. choose units of analysis by developing analogies among differences/distances;
3. determine the relevant differences/distances; and
4. figure out the contexts in which the theory works.

Or consider this slightly more elaborate advice to graduate students:

1. begin with a critically important political issue;
2. become theoretically attuned to the scholarly ideas breaking around the topic;
3. in the pivotal phase of your work, locate the central puzzles or problems that have prevented a breakthrough;
4. translate your solutions into highly structured, explicit arguments;
5. rather than using a readily available, one-size-fits-all dataset to investigate all sorts of plausible rival hypotheses, carefully execute multiple data collection procedures tied to the problem and solution at hand;
6. using a patient approach to statistical analysis, pay attention to the subtleties of context–theory–evidence connections; and
7. combine case studies, large-N statistical analyses, and formalized theory to allow the big picture to emerge.

There are probably as many such algorithms as there are scholars in comparative politics. And just as different sciences lend themselves to different scientific methods, the different research domains of comparative politics lean on different algorithms. Comparativists quite rightly wed strategies of discovery, explanation, and evidence that best address their particular subject matter.

Comparative politics has been influenced by the many theories of scientific explanation that appeared in the last century, and this chapter has explored the bits and pieces of these approaches. Nobody should have expected the ways of thinking and working discussed here to cohere into a single package offering a universal and generative core of scientific rationality – The Seven Pillars of Social Scientific Wisdom or the Ten Habits (Best Practices) of Highly Effective Comparativists. Anyone skeptical about unified social scientific paradigms should have also been skeptical about integrated paradigms of science. We need to appreciate the power of various abstract approaches to science, but we also need to understand their real-world richness, heterogeneity, and complexities.

Nevertheless, a certain way of thinking and working causally with models and mechanisms, one that economists often use to package the diverse issues of discovery, explanation, and evidence, has recently attracted a great deal of attention (Lichbach 2003: chap. 10). Some comparativists address problem situations where a phenomenon X is causally relevant to another phenomenon Y. These comparativists are motivated by various observations and claims: Variable X is correlated with variable Y; X increases the probability of Y; if X had not

occurred, Y would not have occurred; X has the capacity to affect Y; or simply X causes Y. They then develop alternative explanatory models of the mechanisms of the situation whose operation and output generate robust regularities, information that abstracts and idealizes known stylized facts about X and Y. Families of models and exemplars of success are carried from situation to situation and come to characterize different traditions of problem solving.

For example, Maravall (2007) explains the accountability and survival of parliamentary governments by placing two empirical findings at the center of inquiry: As economic conditions worsen, the probability of a prime minister's electoral defeat increases; as the economy improves, the probability of a prime minister's replacement by fellow politicians in parliament or the cabinet increases. The idea that voters and politicians react to economic conditions differently, that executives can be replaced by elite conspiracy and by mass voting, is a core conundrum that can be explained by a set of available rational choice mechanisms. Moreover, the theory can be buttressed by statistical analyses designed to establish causality. The nexus of electoral/non-electoral threats to survival in office holds implications for discovery (big normative problems of accountable government and little rationalist puzzles about democracy), explanation (concepts and institutions of representation), and evidence (stylized facts and stories). Maravall has created a nice causal package, but it is also possible to create a nice positivist package. Taagepera's (2007: 693) "macro-Duvergerian agenda" for electoral systems is a clever flow diagram of mathematical relationships. Once the parameters are estimated, comparativists can describe and predict, calculate and replicate known facts about observable phenomena in the world. Ostrom (2007) offers unification as a third type of package. Taking collective action to be the core subject matter of political science, her framework (Figure 8.2, p. 202) uses a general theory of human behavior to integrate structural variables that can affect the likelihood of cooperation through the core relationships of reputation, trust, and reciprocity. Comparativists have thus adopted three styles of scientific explanation – causal mechanism, positivist, and unificationist. Thinking in the middle of things and working to fashion a compelling story, most treat the styles as complements rather than substitutes.

While nowadays just about everyone wants to be a comparative comparativist, comparing concepts and cases, pragmatic comparativists want to transcend a battle of the paradigms and creative comparativists want to use the battle to generate novel and imaginative research moves. This chapter has stressed the latter approach, urging young scholars to value paradigm wars and to be creative in their use of the controversies surrounding paradigms. Budding comparativists should learn how to agree and disagree in comparative politics in ways that foster creative and productive research directions. The strongest antidote to the normal science of variable analysis is to think through a problem situation with multiple paradigms of inquiry and thereby open up ambitious lines of investigation.

We can draw a parallel to social choice theory and the theory of voting (Schofield 2006: 259). Think of the battle of the paradigms as a three-player

game. Research moves are strategy combinations of the game's underlying dimensions. If the three paradigms or schools interact in an anarchic fashion, chaos results. Unless we impose some structure on the debate – rules for discovery, explanation, and evidence – conclusions will wander anywhere. Core or foundational beliefs about discovery, explanation, and evidence – the heart of the science of comparative politics – can limit chaos. Heresthetics involves inventing new dimensions of competition among the paradigms and new rules to structure the competition. Reformists tinker with an existing dimension of disagreement among the paradigms and try to make a creative research move. Revolutionary thinkers – those whose ideas are completely original – invent new dimensions on which paradigms diverge. With their new rules, revolutionaries shake up intellectual space.

We can also draw an analogy with Kantian/Weberian thought. Academics know the parts – rationality, culture, and structure – but we do not know the whole, which remains mysterious. We can grasp the alternatives, but we cannot combine knowledge into a comprehensive account of truth. We know why each part is unsatisfactory, but we cannot achieve complete knowledge. Rather than attempting a do-it-all or know-it-all synthesis, we thus study one thing at a time, try to think deeply about it, and then move on to the next thing. Valiantly holding everything else constant, we boldly attempt to critically evaluate the result.

Whether one adopts a rationalist or a Weberian metaphor for thinking and working in the midst of things, comparative politics has been, is now, and will forever remain a multiparadigm field. Get used to it. It's paradigms all the way. While effective combinations and practical convergences have emerged, differences and tensions remain. The field has hoped for unity, but the parts never seem to align properly. Since we are a synthetic discipline, ideal-type paradigm thinking is needed to provide the components of the latest and greatest pragmatic attempt to pull things together. If paradigmatic thinking is ruled out of the scholarly domain called comparative politics, it will surface elsewhere in political science and in the social sciences. Comparative politics then becomes parasitic as well as synthetic.

"What this country needs is a good five-cent synthesis," shouts Moses Herzog. Betwixt and between, Saul Bellow's modernist-plagued academic is alienated from tradition. New York City's liberalism, pluralism, and multiculturalism offer no home, and he needs to make sense of his life.

The Joker, that fun-filled postmodernist, lives in and for Gotham City. About to be bested by his rival, Batman, Jack Nicholson delivers the memorial line, "Where does he get those wonderful toys?" Note that the Joker does not beg, borrow, or steal Batman's toys. Although both confuse good and evil, each can distinguish his gadgets from his opponent's gadgets, thereby appreciating gamesmanship when he sees it.

The field of comparative politics, a lot like New York and Gotham, is awash in the chaos and complexity of research traditions, theories, methods, and exemplars. Some academics display modernist angst and aim to forge a final synthesis. The postmodernists play with their toys, and to them a paradigm is

another toy. A third way out of foundationless inquiry is explored in this chapter: pragmatic empirical research in the messy center of comparative politics. Scientific rationality cannot begin with a firm starting point. While entailing illusions and prejudices, discovery allows comparativists to accept their historically situated knowledge, the preunderstandings and prior beliefs (including paradigms) that they bring to inquiry. Explanation thereby entails bold conjectures addressing problem situations. Comparativists then offer evidence that criticizes and attacks, updates and revises, our claims. In an uncertain and insecure world, this pilgrim's progress, this pragmatic way of thinking and working in the midst of things, offers postfoundational comparativists the courage to pursue truth, beauty, and justice that work in theory and in practice.

Social science is a serious business – so we also need to have fun. If the discussions of rationality, culture, and structure in this volume help comparativists use one or more paradigms heuristically and thereby discover a fun difficulty, suggest a fun explanation, and provide some fun evidence, we will have done our jobs as teachers of comparative politics. We advance science by treating research tools as toys, which is why we always end our articles and books with calls for more research, for having more fun thinking and working in the midst of things.

Advancing Explanation in Comparative Politics

Social Mechanisms, Endogenous Processes, and Empirical Rigor

Alan S. Zuckerman

In comparative politics, explanations should join social mechanisms and empirical analysis. The more reason to accept the explanatory value of the social mechanisms and the greater the validity of the empirical work presented, the more reason there is to accept the explanation. This general statement modifies the opening line of my chapter in the book's first edition, "In comparative politics, theory seeks explanations for sets of political phenomena" (Zuckerman 1997: 278). Here, I replace explanation with social mechanisms for those with covering laws and simple causal accounts, and I also insist on the need for rigorous empirical work in substantiating explanatory accounts. The change indicates both more modest and more demanding requirements for scientific understanding in comparative politics.

The chapter in the first edition argues for the need to develop new standards for explanation in comparative politics. Drawing on work in the philosophy of science, in that chapter I displayed flaws in the subfield's two most widely accepted modes of explanation—covering law (also called "deductive-nomological" or "hypothetico-deductive") and causal explanations. Some of these weaknesses derive from the formal properties of these modes of explanation, and these are well known to philosophers of science even if they are less well understood by comparativists. Others, I suggested, result from the clash between the simple ontology of linear and frequently determined relationships, which accompanies each of these explanatory tropes, and the nature of the political world, which includes endogenous, nonlinear, and stochastic relationships. Each of these statements, I maintained, affects the research schools in comparative politics; each implies the need for fresh combinations of explanations. Seeking to move comparativists toward new ways of thinking about scientific understanding, the chapter displays the implications of alternative ontologies of the political world for modes of explanation.

My thanks to Mark Lichbach, Margaret Levi, Ira Katznelson, and Marc Ross for comments on earlier drafts of this chapter. This chapter is dedicated to the memories of my nephew, Mark I. Davidman, obm, and my cousin Tamara Engel, obm, both of whom died while I was working on it. May their memories be blessings for their families and friends.

No matter the chapter's accomplishments, it contains flaws. Most comparativists, like other political and social scientists, equate explanation and causal analysis,¹ ignore or misunderstand covering-law explanations, and employ a slew of related but not necessarily congruent verbs to indicate explanatory claims (for example, "cause," "influence," "affect," "predict," "account for," "provide theoretical insight," and "model"). As a result, that chapter's effort to differentiate the two modes of explanation speaks to formal principles but not to the actual practice of research in comparative politics. Also, the chapter does not prescribe new explanatory standards to accompany its claims about alternative ontologies. As a result, whatever its success as a description of how comparativists explain the political world, it fails as a homily.² More, like all sermons, it offers a verbal message, not evidence that the message works in practice. Because of these and perhaps other flaws, it is difficult to find the chapter's impact on subsequent research in comparative politics.

In this chapter, I strive to correct these errors. Explanations in comparative politics, I maintain, require social mechanisms that account for and empirical evidence that detail the specified explanatory process. As important, the ontology of politics demands explanations of stochastic, multilevel, and endogenous phenomena, so simple one-directional causal claims are insufficient (Przeworski 1995, 2007; see also Anderson's, Huckfeldt's, and Rodden's chapters in this volume). Applying appropriate statistical techniques transforms the language of explanation from imprecise verbal accounts into precise formulations, permitting the evaluation of the fit between abstract claims and the empirical evidence. As an effort to convince by showing, I illustrate my message by applying it to research on partisanship and political violence.

My presentation draws primarily on work that has appeared since the publication of the book's first edition. The chapter opens with prescriptions for explanation in comparative politics. In the next section, I argue that such explanations require social mechanisms. After that, I explore new ideas for social mechanisms, drawing on theory and findings from across the social sciences, and then I apply some of these innovations to research on partisanship, voting, and political violence. The following portion explores general principles of empirical research and relates them to explanatory research in comparative politics. The conclusion returns to the theme of successful explanations in comparative politics.

¹ This equation may be difficult to resist. Drawing on a broad array of research in cognitive science, Sloman (2005) argues that searching for causal relationships underpins how humans try to understand the world. Defining explanation by causality is a deep intuition. Indeed, it would seem that only a sustained effort by especially conscious thinkers prevents this expectation from being attached to all explanations. It is not, therefore, just one way to understand the world. Brady (2008) offers an extended defense of the principle of equating causal and explanatory analysis in political science.

² Brady's analysis (2004) of King, Keohane, and Verba (1994) highlights how descriptions of research become prescriptions. The point applies no less to this chapter.

EXPLANATIONS IN COMPARATIVE POLITICS

Comparative politics, in particular, and political science, in general, seek to explain political phenomena.³ Indeed, scientific understanding distinguishes social science from other attempts to know the social, economic, and political worlds, just as it separates science from other forms of knowledge.

In principle, an explanation is an account of why an event (or a set or series of events) occurs (for the classic statement, see Hempel 1965). The effort links the explanandum (that which is to be explained) and the explanans (that which explains). A successful explanation does more than establish statistical or other forms of association and impact (as is expected of causal accounts), and it entails more than the ability to predict unknown from known phenomena (an expectation of covering-law explanations); it requires well-established theoretical principles. These sustain the expectation that the proposed explanatory propositions will answer the particular explanatory question at hand.⁴ A successful explanation also claims veridicity: The abstract world of ideas describes events and behavior, that is, it demonstrates empirical accuracy or goodness of fit. As a result, explanations in principle vary from the implausible (on grounds of theory) and/or the mistaken (with regard to goodness of fit) to the demonstrated (where strong theory merges with empirical verification to provide theoretical and empirical certainty). Implausible and wrong explanations must be rejected; those that are demonstrated must be accepted.

Examining the subfield's research schools – rational choice theory, structural and cultural analyses (see especially Katznelson 1997; Lichbach 1997a; Levi 1997a; and Ross 1997 and their chapters in this volume) – displays their guiding questions, answers, and modes of empirical analysis. Taken together, they describe comparative politics as a field that examines the bases of political choice, the determinants and effect of large processes – such as regime structures, party systems, market forces, and revolutions – and the relationships among preferences, values, and calculations on these phenomena. Observational studies also characterize comparative politics. Explanation is always applied to particular polities, even as the field strives to apply abstract arguments and models.

In practice, explanations in comparative politics do not compel their acceptance.⁵ No laws of politics apply the power of deductive logic to establish their truth claims. Research always begins with explanatory claims whose prior

³ See Hedström (2006) and Hausman (2001) for discussions of explanation in sociology and economics, respectively. Models stand as the alternative to explanations as the centerpiece of scientific research. Where they are not a specific mode of explanation, however, they lack clear standards for success and failure. For recent examples of arguments that present models as alternatives to explanations, see Clarke and Primo (2007) and Miller and Page (2007).

⁴ See Ioannidis (2005) for a restatement of this principle, applying it to medical research: “[t]he probability that a research finding is indeed true depends on the prior probability of it being true (before doing the study),” as well as measures that display strong association and statistical significance (c124).

⁵ A discussion of the significance of careerist, friendship, and other such reasons for accepting or rejecting explanations is outside the scope of this chapter.

probabilities of success are less than certain. As a result, empirical work is caught in the paradox of conceptualization: Absent verified theories, we cannot know if concepts cut at “nature’s joints,” to return again to a theme in Aristotle. Doubt always applies to their explanatory value. And so, definitions, no matter how precise they are, do not establish the concept’s analytical significance. Carefully delineated measures are not the same as perfect measures, and so there is always a gap between the concept’s abstract words and the empirical measures.⁶ More, refining research methods cannot remove these uncertainties. All statistical models vary in utility; none is perfect. Also, simply refusing to employ quantitative methods surely cannot guarantee success, and neither can adding together different methods. In sum, the absence of certain theoretical claims limits the strength of all explanations, and the absence of definitive empirical tests mars all efforts to display goodness of fit between the theory’s virtual world and the phenomena to be explained. It follows that the “mean” study in comparative politics presents ambiguous results with regard to theory, concepts, measures, and analysis of the empirical evidence.⁷

What places an explanation close to the demonstrated pole? What moves an explanation along the scale, thereby increasing the likelihood of acceptance while decreasing the likelihood of rejection? I offer a simple and direct answer: The more that it includes explanations derived from verified social mechanisms, the more that these mechanisms account for people’s decisions, the more that the explanation is able to account for endogenous relationships, and the greater the number of demanding empirical tests, the more reason there is to accept the explanation. Successful explanations narrow but cannot eliminate the gap between the ideal and the real.

EXPLANATIONS WITH SOCIAL MECHANISMS

Causal mechanisms seek to account for an observed relationship between a proposed cause and effect (for applications to comparative politics, see the chapters by McAdam, Tarrow, and Tilly and Lichbach in this volume, as well as McAdam, Tarrow, and Tilly 2001, 2007; and for a more general statement see Elster 1998; Gambetta 1998; Hedström and Swedberg 1998a; Hernes 1998; Schelling 1998).⁸ Unlike covering laws, the premises of the arguments in which they appear do not entail the truth of the conclusion. These propositions cannot predict specific outcomes; instead, they account for general processes or probabilistic outcomes.

⁶ See Ecco 1994 for a funny and, therefore, particularly telling elaboration of this well-established principle. With particular regard to comparative politics, the inevitable gaps between concepts and their measures raise warning flags about the use of datasets that cover many countries, without appropriate assessments of the internal validity of the measures for each data point; see the following discussion.

⁷ See Ioannidis 2005 for an extension and formalization of this claim regarding medical research.

⁸ McAdam, Tarrow, and Tilly alter the focus slightly, emphasizing the impact of mechanisms on outcomes. They define mechanisms as “a delimited class of events that alter relations among specified sets of elements in identical or closely similar ways over a variety of conditions” (2001: 24).

These sometimes true propositions offer a partially successful response to the need to demonstrate “constant conjunction” and to avoid spurious claims in explanations. They explain by providing a principle that usually accounts for the observed relationship. As a result, they vary in the prior probability of explanatory success, and they do not necessarily address the need in comparative politics to analyze the interactions among macrostructures, microprocesses, and individual behavior.

Social mechanisms are a subset of the general category of mechanisms, distinguished from the others by applying the principle of methodological individualism. They specify what people can do and what they know and value, as well as their attributes. They also include the social contexts in which people live their lives, the correlates of these roles or social states, and the distributional features of these social locations in the explanatory propositions. In turn, they relate the two levels of analysis, examining the inherent endogeneity of economic, social, and political processes (see especially Hedström and Swedberg 1998 and Hernes 1998).

Analyses of political choices, their cause, effects, and relationships with other political phenomena, must include social mechanisms. Analyses of macroprocesses must also include social mechanisms when they employ motives and choices. Only explanations that purposefully ignore choice processes may omit social mechanisms. As most, if not all, analyses in comparative politics include political choices, explanations with social mechanisms answer the requirements of explanation in comparative politics. Indeed, they are the “prose” of social science.⁹ Linking social mechanisms and explanations in comparative politics speaks not only to principled issues of the philosophy of science, but directly to the ways comparativists work: explanation in practice as well as principle.

Social science abounds in social mechanisms. Some derive from the wisdom of ancient Greece;¹⁰ some come from centuries’-old French philosophers; others from recent work in decision theory, experiments, agent-based modeling, and neurobiology.¹¹ Homilies that urge social scientists to use social mechanisms

⁹ See the following discussion of the research by a group of social scientists who offer a similar framework that focuses on “beliefs, preferences, and constraints” (Henrich et al. 2005b: 841). Manicas (2006) offers a philosopher’s formal recognition of these principles for explanation in social science. To echo Moliere: Explanation with social mechanism is the everyday language of social science, whether or not those who speak it are aware of it.

¹⁰ Aristotle offered the principle of homophily, or “like to like,” and its converse, “opposites attract.” See Zuckerman, Dasović, and Fitzgerald (2007) for an intellectual history.

¹¹ Volumes of research across the social sciences sustain this position: Cunningham and Barbee (2000: 274) and Laland (2001: 233) use the principles of social psychology; Axelrod (1997a, 1997b), Fowler (2005), Huckfeldt, Johnson, and Sprague (2004, 2005), Johnson (1999) and Johnson and Huckfeldt (2005) apply agent-based models, and Elster (2005) draws on the principle of French moral philosophers of the eighteenth century.

Recent research in neuroscience makes it possible to disentangle social mechanisms from conscious choice. Dijksterhuis maintains that imitation is the default mode of social learning: “we are wired to imitate (2005: 218).” In turn, Kinsbourne 2005 associates imitation with affiliation or attachment even more than with learning. Parallel studies find sources for social learning in physical processes located in particular portions of the brain (see for example Berns, Chappelow, Zink, Pagnoni, Martin-Skurski, and Richards 2005; King-Casas, Tomlin, Anen, Camerer, Quartz, and Montague 2005; Kinsbourne 2005; and Porges 2005).

include specific recommendations. Schelling displays the analytical importance of contagion models and “self-realizing expectations,” also known as “self-fulfilling prophecies” (Schelling 1998: 34–35, 38–40). Elster presents sets of opposites. The first considers efforts to reduce cognitive dissonance by “wishful thinking” or “adaptive preferences” (1998: 53). The second examines three different effects as social mechanisms: “compensation” (“*envy of one’s enemies immunizes against envy of friends*” italics in the original), “spillover” (participation at work increases the likelihood of political participation), and “crowding out” (participation at work takes time away from other forms of participation; 1998: 54–55). Gambetta adds: “instrumental rationality, focal points, biased inferential processes; . . . self-validating beliefs such as distrust; emotions such as envy; passions such as *amour propre*; evolved dispositions towards altruism, sex, or children, special cognitive quirks like the endowment effects, and so on” (1998: 103). Even as they deny the utility of methodological individualism, McAdam, Tarrow, and Tilly classify three types of social mechanism for the analysis of contentious politics: environmental, cognitive, and relational, each with numerous examples (2001: 25–26; see also McAdam, Tarrow, and Tilly 2007). As social mechanisms proliferate, lists and classifications form, and with that comes the need to distinguish among them.¹²

Social mechanisms relate directly to the research schools of comparative politics. The principles of both classical Marxist theory and rational choice theory are easily transformed from candidates for covering laws into social mechanisms. Positing collapsed actors (Hernes 1998), Marxist theory insists that persons take on the interests of their social class and act to extend those interests. Rational choice theory begins with rationally calculating persons who seek to maximize their self-interest while minimizing costs. Lichbach’s, Levi’s and Blyth’s chapters in this volume highlight the movement of rational choice theory away from the assumptions of neoclassical economic theory: replacing assumptions of optimization and self-interest with thin rationality and an emphasis on altruism and other forms of behavior in accord with ethical principles. Katznelson’s (1997) and my chapters (1997) in the first edition of this volume and Huckfeldt’s, Anderson’s, and my chapters in this one modify the determinist claims of classical structural analyses. Similarly, Katznelson (1997), Ross (1997), and my (1997) chapter in the first edition, as well as Huckfeldt’s, Anderson’s, and my chapters in this edition, modify the holism that was long associated with the culturalist perspective. Both editions illustrate how the research schools of comparative politics apply explanatory principles used across the social sciences.

Social mechanisms vary in theoretical and evidentiary support and, therefore, in the prior probability that empirical analysis will match their claims. Indeed, advancing scholarship requires both testing and justifying the social mechanisms and the results obtained by applying them. The more reason and evidence that support a social mechanism, the greater is the prior probability that the explanations in which it is used merit empirical tests, and the greater is the likelihood that they will succeed.

¹² Steel (2004) offers tests for the evaluation of social mechanisms in causal inference.

During the past decade, studies that draw on abstract reasoning, agent-based models, laboratory and field experiments, and ethnographies have provided reasons to accept, modify, and reject particular social mechanisms. I will display results that challenge the principles of optimization and selfishness; distinguish between instrumentalist decisions and those that derive from social processes, and emphasize the endogeneity of preferences, identities, and immediate and more general social and cultural contexts. This research shows how social and political contexts interact with what people know, believe, and value, as they make political choices.

FROM OPTIMIZATION AND SELFISHNESS TO BOUNDED RATIONALITY, HEURISTICS, AND SOCIAL LEARNING

Building on Simon's classic work on bounded rationality, an array of scholars from economics, sociology, decision theory, and political science reject optimization as a principle for accounting for choices. Instead, they maintain, there is strong reason to expect people to make what they perceive to be the correct decision, even if they do not succeed and even if the decision is not objectively correct (see Boudon 1992, 1998; Gigerenzer and Selten 2001b; Jones 2001; and for the classic sources on bounded rationality see Simon 1957, 1999). They seek strong reasons for what they do (Boudon 1992, 1998).

According to bounded rationality, how do people make decisions? People make the subjectively rational choice. Usually eschewing complex calculations, people apply useful rules of thumb – or usually correct theories or understandings or decision heuristics. Some of these decision rules characterize persons as asocial beings, for example “repeat the most recent choice” – otherwise known as the “status quo bias” (Samuelson and Zeckhauser 1988) and the “endowment effect” – people prefer not to trade what they have (Thaler 1980). Some, like “imitation,” “social learning,” and “the application of social norms,” imply learning from other persons. Consider a set of decision rules taken from recent research on decision theory that sustain the principle of social learning:

[I]n social species, imitation and social learning can be seen as mechanisms that enable fast learning and obviate the need for individual calculations of expected utilities.

Social norms can be seen as fast and frugal behavioral mechanisms that dispense with individual cost-benefit computations and decision-making. (Gigerenzer and Selten 2001a: 9–10)

Social imitation can help make decisions with limited time and knowledge. Heuristics such as “eat what your peers eat” and “prefer mates picked by others” can speed up decision making by reducing the time spent on information search. (Selten 2001: 48)

Imitate if Better, assumes that individuals imitate all others who are more successful than themselves, and stick with their current strategy otherwise. . . . Proportional Imitation . . . dictates imitating those who are more successful than oneself with a probability that is proportional to [the] difference between the observed and current degrees of success. (Group Report in Gigerenzer and Selten 2001b: 175)

Social learning applies: Boundedly (or subjectively) rational actors proceed by learning from others. This principle differs from the expectation that people will evaluate all possible options, seeking to make the objectively best decision.

Other social scientists limit the principle of selfishness (people seek to maximize their self-interest). Henrich et al. (2001, 2004, 2005a, 2005b)¹³ describe the research of a team of 15 anthropologists, economists, psychologists, and complexity theorists on responses to a series of experimental games in 12 small-scale societies across the world, 11 of which are located below the Equator, with comparisons to previous work among Americans.

Here is their summary, as it relates to selfishness:

Our primary interest was to explore the between- and within-group variation in other-regarding social behavior across a wide range of small-scale societies. Our experiments produced evidence inconsistent with the selfishness axiom. (2005b: 839)

Selfishness, they clarify, is not the same as rationality, which they “use to mean consistency in behavior *without any attribution of selfishness*. People can be rational and still care about equity, or care about others” (2005b: 839–840). Taken together, this research substantially modifies fundamental principles of classic rational choice theory. Social learning stands as another mode of reasoning, and selfishness must be joined by other preferences.

FROM EXOGENOUS TO ENDOGENOUS PREFERENCES AND IDENTITIES

Consider now the matters of preferences – what people value – and identities – how they are defined, both critical to social mechanisms that account for behavior. Whereas classical rational choice theory views them as exogenous – theoretical primitives that do not respond to other factors – and classical structural and culturalist perspectives reduce them to the preferences and identities of their social classes, ethnicities, or other large-scale contexts that do not respond to individual actions, there is increasing reason to treat them as phenomena whose variation is endogenous to social locations.

Returning to Henrich and his colleagues’ research, I begin with their summary statement:

[P]references over economic choices are not exogenous as the canonical model would have it, but are shaped by the economic and social interactions of everyday life. (2001: 79)

Their more elaborate presentations (Henrich et al. 2005a, 2005b; see also Henrich et al. 2004) develop this point. These show that group-level measures that describe the particular economic and social context (“society, village, camp, or other subgroup membership”) account for between-group differences in the

¹³ Reflecting the importance of Henrich et al.’s (2005a) research, the editors of *Behavioral and Brain Sciences* published the peer reviews of 21 scholars from anthropology; economics, decision research, psychology, and political science. Henrich et al. (2005b) is the authors’ response to the reviewers’ comments.

responses to the experiments. Individual-level differences, as measured by sex, wealth, and age, do not (Henrich et al. 2005a: 805–807). More generally, they argue on behalf of a research agenda that explains by examining interactions among variables that assess individual and contextual phenomena.

Within economics, this research resonates with Arrow's insistence that the possibility of rational behavior is endogenous to the structure of the macroeconomy. Individual rationality, he contends, is not only a property of each person. It is linked to the presence of an economic equilibrium, perfect competition, and "completeness of markets":

When these assumptions fail, the very concept of rationality becomes threatened, because perceptions of others and, in particular, their rationality become part of one's own. (1986: S389)

It meshes as well with other research that maintains that economic preferences respond to features of the general economy (Bowles 1998; Heintz 2005; Zizzo 2003, 2005) and with Etzioni's (1985) early argument on behalf of the endogeneity of economic preferences.

Scholarship in sociology and anthropology also underscores the interactions between individuals and their social contexts. Challenging Weber's classic view of culture as causally coherent and determinant, Swidler underscores the interaction between the elements of a culture and people's ability to choose from among the items of knowledge, beliefs, and preferences of their culture:

Culture constrains action because people can most easily construct strategies of action for which they have the cultural equipment. When people develop lines of action, they start with the repertoire of things they already know. Implicitly, they ask, "Who am I? What am I good at? What do I know?" (2001: 84)¹⁴

Swidler offers culture as a "toolkit" from which individuals pick answers to these questions.

Standing apart from the culturalist's classic tendency to equate what people know, desire, and value with the general value and knowledge systems of their culture, Mary Douglas defines humans as inherently social, necessarily interacting with and learning from each other:

As a social being the person needs to be capable of reading messages from other persons, or responding to these and of composing intelligible messages to send out. . . . This person has to have beliefs about how the world is and how it works, ontological knowledge, and knowledge about how other persons behave. . . . Equipped with the wherewithal to make choices, the rational social being will apply choice to dealings with other persons and will develop strategies for manipulating and controlling them and for escaping unwanted control. (Douglas and Ney 1998: 89)

Culture contains a set of implicit and explicit expectations that influence individual choices but are also open to personal manipulation. "Thinking is possible, because

¹⁴ Note the parallels to the characterizations of the individual in Hernes (1998) and Henrich et al. (2005a) presented previously.

it is stabilized by shared practices; social life is possible, because it is stabilized by thinking” (ibid.: 31). In this perspective, people do not stand apart from their social relations, choosing one or the other for instrumentalist reasons. Rather, social relations are integral to what it means to be a human.

Other sociologists also expand the scope of the discussion of endogeneity between choice and structure by moving beyond the economists’ definition of preferences as motives to address broader issues, including knowledge, general values, and specific attitudes. Consider Zuckerman’s and Turco’s (2008) analysis of identity. Drawing on recent advances in the study of social networks as well as the classic scholarship of Cooley, Meade, and Simmel, they maintain that identity – defined as “consistency in placement” – is not a self-referential choice. Instead, it responds to audience decisions, that is, other people define one’s identity. Members of the audience, however, are limited in how they can define the subject:

The general point is that common knowledge of a code creates pressure to use the code insofar as decisions are reviewed by others who share the same common knowledge. And this tendency to use the code even by choosers who do not privately subscribe to it makes such codes and identities resistant to change. (Ibid.: 13)

In their analysis, identity responds to three sets of factors: past behavior, placements by persons in one’s immediate social context (“audience”), and variations in the acceptance of a common set of understandings. Consistency in action, therefore, is endogenous to identity (ibid.: 23). In this perspective, identity is neither a “self-concept,” as many economists would have it, nor an identification, as it appears in studies of attitudes in social psychology and political science.

Taken together, these sources suggest that as people interact with others at work, at home, at play, and in other encounters, they come to know who they are, what they value and understand. In turn, this set of knowledge, preferences, and attitudes affects their social interactions. The sources surveyed agree on the importance of social learning. People learn from each other, as they also select elements of the culture that help to fulfill their needs and desires, and these in turn are endogenous to their social circumstances. These provide well-supported social mechanisms for the analysis of political choices and behavior.

SOCIAL LEARNING AS AN INSTRUMENTAL CHOICE AND AS A SOCIAL PROCESS

In these endogenous processes, how do people choose? Scholarship does not agree on the social mechanisms that influence this process even when it agrees on the importance of social learning. Echoing differences between rational choice theory and structural and cultural analyses, a critical distinction separates social learning as an instrumental choice – people learn from others because it is an efficient means toward their goals (a rational choice) – from social learning as a social process that implies a justification – people learn because that is what humans do, especially when they need to justify that choice to others. When

social learning is instrumental, people follow those who know more than they do and can teach them how to move toward their goal. With regard to political choices, they seek information from persons better informed about the issue at hand. When social learning is not instrumental, deriving from ongoing social interaction, people follow those with whom they interact frequently, who are like themselves, whom they admire, love, trust, to whom they feel accountable – among possible bases for cue-taking. When cue givers disagree with each other, neither approach offers a single answer to the pattern of influence. In these cases, cue takers may follow the net preferences of cue givers; they may weight them on an easily constructed scale; they may follow the cues of one person chosen for one or more of the reasons noted previously. Instrumentalist social learning implies hierarchical relationships – influence flows in one direction – whereas social learning draws on social processes, in which influence is reciprocal – each person influences each of the others. The different positions clearly imply different modes of empirical analyses, as well as explanatory relationships.

Examining social learning with regard to political choice helps to clarify the utility of social mechanisms for explanations in political science. First, I explore analyses that apply both types of social learning to partisan decisions in established democracies. The next discussion examines the social mechanisms of political violence, where instrumentalist assumptions without social learning abound. The discussion focuses on how to account for choices and highlights efforts that approach political decisions as endogenous to social circumstances. It offers, therefore, a selective interpretation of wide-ranging literatures.

SOCIAL MECHANISMS IN RECENT RESEARCH IN COMPARATIVE POLITICS: PARTISANSHIP AND VOTING

I start with a description of the theoretical principles in my recently published book, offering it as my exemplar of a successful explanation in comparative politics that uses social mechanisms, hoping to sustain the value of this homily. My colleagues and I pursue the principles of endogenous social influence on partisanship and voting in contemporary Germany and Britain (Zuckerman, Dasović, and Fitzgerald 2007). People make decisions about political parties, we reason, much as they make other decisions. They take into account the preferences, values, expectations, and perceptions of their family, friends, colleagues, and neighbors. People affect one another, and so any one decision responds to the particular mix of views in a person's social networks. As most persons live with others and as they usually trust those with whom they live, members of their households – usually husbands, wives, parents, and children – influence each other's political decisions. Furthermore, each of these immediate social processes also responds to more distant social positions – especially those of social class and religion. In place of social mechanisms that draw on instrumentalist assumptions, we apply a “social logic.”

Our analysis recognizes another element of complexity. We also analyze partisanship (composed of partisan support – for any political party; partisan preference – for a particular party; and the consistency of these choices over time)

and voting (turnout and casting a ballot for a particular political party) as sets of related choices. Clearly, partisan preference requires partisan support and vote choice presupposes turnout. We also expect this social process to be endogenous to political structures; the distribution of partisan preferences influence strategic decisions of party leaders, and electoral mobilization affects partisan preferences. The volume's conclusion reasons about the place of electoral mobilization by the political parties in this process, adding another level of analysis (and see Anderson's and Huckfeldt's chapters in this volume for more detailed discussions of this point). Our social logic applies social mechanisms that account for a series of endogenous and complex choices about partisanship and voting.

Consider how our analysis differs from others that also apply social learning to partisanship but keep assumptions of instrumental choice and optimization with regard to time and sources of information (drawing primarily on Downs 1957). Like Huckfeldt and his associates (for example, Huckfeldt 2001; Huckfeldt and Sprague 1995; Huckfeldt, Johnson, and Sprague 2004), we examine the impact of members of social networks on each other, but we do not expect influence to follow political knowledge in these relationships, and so we need to model endogenous relationships and may not be satisfied with one-way causal relationships. Like Lupia and McCubbins (2000), we suppose that social learning requires trust, but we believe that it is both more parsimonious and more accurate to look to loved ones – parents, spouses, and children, as well as friends and work mates – than to political institutions for sources of trusted information. Like Achen (2002a), we accept the principle that persons are not passive receptacles of political messages. Like Achen too, we maintain that persons learn from those around them. We differ, however, with regard to the mechanisms of social learning regarding partisanship. For Achen, children, like everyone else, take their understanding of desired political benefits from their social position, and they support the party that they calculate is in their future self-interest. Seeking to maximize their expected benefits and supposing that they will occupy the same social position as their parents, young people take their parents' partisanship. Children learn partisanship from their parents, we suggest, because they interact frequently and take many cues, among them political ones, from their parents. Furthermore, as children become young adults, they teach their parents too. In contrast to our study, these studies of partisanship and voting in established democracies combine social learning with the principles of optimization and self-interest – instrumental choice.

SOCIAL MECHANISMS IN RECENT RESEARCH IN COMPARATIVE POLITICS: POLITICAL VIOLENCE

Studies of political violence seek to account for political processes in which people injure and kill each other, where these actions are formally illegitimate and outside the law. They examine a far more demanding topic than partisanship and voting. Here, the analytical needs of comparative politics merge with the human desire to understand (and perhaps even to prevent) these awful events.

Successful explanations of political violence, therefore, hold out the promise of improving the world, a patently valuable goal, even if it lies beyond my formal limits for explanation in comparative politics. Responding to both sets of demands during the past decade, comparativists have authored numerous studies of communal and ethnic violence, killings of civilians in civil wars, as well as other modes of political violence. Their work joins the scholarship of students of international relations, as well as that of historians and sociologists.

How and why do people use violence to extend their political will over others or to defend their political position? Consider first answers that apply a social mechanism that assumes an instrumentalist perspective on political choice.¹⁵ Kalyvas summarizes the position: “Political actors use violence to achieve multiple, overlapping, and sometimes mutually contradictory goals,” and then cites numerous examples from the literature on political violence (Kalyvas 2006: 23). Sometimes, leaders of states attack civilians in order to defend the state or the regime (Midlarsky 2005; Valentino, Huth, and Balch-Lindsay 2004), and leaders of ethnic groups seek to defend their group from expected attacks (Posen 1993b). Here the “security dilemma” – kill or be killed, linked to perceptions of threat and applied to abstract entities or collectivities, but not necessarily to actual assaults on particular persons – motivates the attacks. Other scholars offer alternative incentives for instrumentalist interpretations of political violence. Drawing on a classic structuralist position as well, Varshney (2002) explains incidences of riots across India as consequences of the interactions among ethnic associations. Applying the literature of political cleavages, he relates cross-cutting ties to communal peace and reinforcing cleavages to violence. Where elites benefit from these associations, they prevent riots. In contrast, Wilkinson’s (2004) study of riots across Indian states links variations in their incidence and duration to the electoral strategies of party leaders seeking to defend their hold on power. Leaders who fear electoral loss induce riots, calculating that it will frighten uncertain supporters into voting for them; those with no such fears allow nonviolent politics to proceed. Similarly, Fearon and Laitin (2000) theorize that ethnic leaders use violence to establish ethnic groups, turning persons who might identify with them into actual members. In various ways, political violence is taken to be a means to a political end.

Like all explanations in comparative politics, analyses of political violence benefit from distinguishing among actors, including multiple levels, and expanding the characterizations of what people know and value. Fearon and Laitin¹⁶ address the need to explain “how elites can convince their followers to adopt false beliefs and to take actions that the followers would not take if they understood what the leaders were up to” (2000b: 853). Reviewing recent

¹⁵ For recent examples that do not assume the principles of methodological individualism and include structural factors without linking them to political choices, see Fearon and Laitin (2003) and Eck and Hultman (2007).

¹⁶ The paper is marred by the bizarre and unfounded claim that one of Hitler’s parents was Jewish (see p. 857). Because it examines analytical themes and not particular cases, this mistake does not weaken the power of their analysis. I return to the impact of factual errors on explanatory accounts later; see especially footnote 24.

constructivist accounts of ethnic violence, they draw together instrumentalist responses and those that rely on discursive analyses with those that emphasize processes. “The strategic theories linking individuals (whether elites or masses) to ethnic violence and the discursive theories linking discourses to violent behaviors . . . all posit the content and boundaries of ethnic groups are produced and reproduced by specific social processes” (874). In this view, ethnic violence is an endogenous process. Other scholars offer a different solution: Successful explanations should recognize that hardly anyone engages in acts of political violence; professionals do (for the classic statement see Moore 1978, and for a formalization see Lichbach (1995a,b). Brass (1997) and Tarrow (1994) highlight this solution. In India, “thugs” for hire, act at the behest of political entrepreneurs. In Italy, professionals in violence reject participation in electoral contests. Both sets follow an instrumental calculation that separates them from most citizens, who do not take part in political violence.¹⁷

Kalyvas examines interactions among regimes, social processes, and the people who engage in violence. Emphasizing the presence or absence of an established political regime, he insists that conflict in war is not the same as conflict in peace. “Conflating violence in the context of contentious violence with civil war violence suggests a failure to recognize that war and peace are radically different contexts for violence that induce and constrain violence in different ways. . . . Contentious action represents a challenge to the government in place in a context characterized by an undeniable monopoly of violence by the state. In contrast, the defining characteristic of civil war is the absence of such a monopoly” (2006: 22–23). Hence, violence in civil wars is fundamentally different than other kinds of violence. Kalyvas also addresses violence where it occurs, at the microlevel: Personal incentives (such as revenge, anger, and greed) provide motives for violence that receive the label “political.” Violence in civil wars reflects not only “the politicization of private life” but also the “privatization of politics” (ibid.: 332; see also Brass 1997, who maintains that much of what passes for communal violence in India is personal crime described with the language of collective politics; and McAdam, Tarrow, and Tilly 2001, who deepen and broaden the general point). Variations in control and violence between incumbents and insurgents interact with each other, as the “intimate” violence among friends and neighbors interacts with the master cleavages that define the conflict at the national level (Brass 1997 and King 2004 also underscore the microcontexts of political violence).

An alternative interpretation completely rejects the instrumentalist assumption while retaining the expectations that political violence incorporates endogenous processes of related choices and that analyses must examine the

¹⁷ There are relatively few direct analyses of the people involved in political violence: those who harm, those who watch as others harm, and the victims. Novelists serve as sources of insight. Here, the literature on victims is large and well known, but the literature on those who actively or passively engage in political violence is also meager. For an exception and a powerful interpretation of those who harm and those who stand by; see the short story “The Lamb” in Nádas (2007).

microprocesses of conflict. Schiemann (2007) articulates this view from within political science. The sociologist and historian Wolfgang Sofsky sets out the general position:

Any instrumental concept of violence fundamentally fails to understand the dynamic of violent processes, which themselves engender their concomitant drives from the first. Violence does not obey the calculations of expediency, and cannot be reduced to the status of a means towards political, economic, or ideological ends or interests. We shall not get a clear idea of most collective crimes if we think that they can be ascribed to the purposes of individual criminals, manipulators, or seducers. (2002: 18)

Instead, Sofsky emphasizes the ease with which ordinary people learn to use violence and its attractions. “[V]iolence itself creates the tendencies that satisfy it. The community of violence has its own charms” (ibid.: 110). His slim volume explores the expectations that members of the police place on each other; the clashing perceptions of time between the perpetrators and victims of police raids; and other processes that approach political violence as a social process. Furthermore, this perspective offers a way to analyze the victims of political violence, treating them as more than objects of other people’s actions.

Social mechanisms that emphasize instrumentalist motives and social processes offer alternative theoretical principles for explanations of partisanship, voting, and political violence. Because research on partisanship draws on social mechanisms with established pedigrees in economics, sociology, and political science, it is easier to make decisions about the prior probability of explanatory success when these mechanisms are used. These calculations are more difficult when applied to political violence. No matter the social mechanisms applied, much of this research examines complex endogenous processes with multiple levels.

APPLYING SOCIAL MECHANISMS IN EMPIRICAL ANALYSIS

Successful explanations using social mechanisms require empirical evidence. Because social mechanisms always vary in strength – in the prior probability of explanatory success – these explanations lack the compulsion of deductive-nomological explanations. They cannot support the argument that the outcome to be explained must occur; they cannot insist that the analysis must be accepted. Moving an analysis toward acceptance requires, therefore, empirical evidence as well as demonstrations of the social mechanism’s high probability of success.

Rigorous empirical analysis defends the validity of the claim of goodness of fit between the abstract argument and the political phenomena to be explained. This effort must demonstrate that the evidence constrains the analysis. Gellner phrases the principle as the distinction between *my* data and *my data*:

The empiricist-positivist tradition tends towards the position that in the end knowledge is and can only be about *my* experience, *my* data. . . . But it all depends on how you read this. Is knowledge about *my* data, or about *my data*? The reason why empiricism clings to experience is not because it is mine . . . , but because it is *outside my control*, because it is

given to me, and there is little or nothing that I can do about it. It is its imposition *on* me, without my consent, not its imposition on *me*, that makes it so valuable as the ultimate arbiter of cognitive claims. (1985: 20)

Rigorous analysis requires techniques that separate scholar and evidence. As scholars apply rigorous and numerous empirical tests to their explanatory propositions, they move the explanatory claim along a scale from plausible toward demonstrated.

Across the social sciences, there is a booming industry devoted to how best to demonstrate the validity of empirical analyses.¹⁸ Here, I highlight four well-known principles of research: (1) the particular explanation should be tested against rival accounts: (a) those that are derived from other social mechanisms and (b) those that respond to contextually specific information; (2) the evidence used – whether in the form of numbers, verbal accounts, or both – must be shown to be (a) reliable and (b) appropriate to the concepts employed in the social mechanisms.

AN APPLICATION TO PARTISANSHIP

A summary of how my colleagues and I apply these principles to the analysis of partisan choice in two European democracies, Germany and Britain (Zuckerman, Dasović, and Fitzgerald 2007), illustrates these themes in my homily. Explanations that derive from principles of social learning should test for both the effects of asymmetric political knowledge and reciprocal influence within households. They should also examine the effects of social mechanisms taken from other theoretical perspectives. Three that describe what people want, know, and value come immediately to mind: the claim of classical rational choice theory that people support political parties that are in line with their personal interests; the view that partisan choice is a form of social identification in which people develop psychological attachments to political parties; and the hypothesis that partisan support derives from attachments to a social class or ethnic group and the political party with which it is associated. Explanation with a particular social mechanism advances in tandem with tests of its alternatives.

Empirical tests need to check as well for the effects of factors that come from observations of politics that are not linked directly to theoretical perspectives. Case-specific information – the analysis of secondary sources and ethnographic research, to offer two examples – has long been at the heart of empirical analyses of politics. Consider some illustrations with regard to partisanship. Our analysis takes into account the secular decline in partisanship that has characterized European electorates over the past two decades. The knowledge that partisan choice peaks at

¹⁸ King, Keohane, and Verba (1994) initiate the most recent discussion of these principles, and Brady and Collier (2004) gather a series of responses and challenges. Donald Campbell offers classic essays designed to teach researchers how to assess the empirical validity of causal accounts (Campbell and Ross 1968 and Cook and Campbell 1979; these precede and cover many of the issues raised in King, Keohane, and Verba 1994; see also Brady 2004 on this point).

elections induces us to contrast responses offered during election years with answers in other years. Also, the expectation that political events and decisions influence perceptions, and therefore the probability of support for one or the other of the political parties, demands that we test for the effects of events that occurred during the years under examination – German reunification, the decline of the Christian Democrats/Socials in Germany and the Conservatives in Britain, as well as the rise of the Social Democrats in Germany and of Labour in Britain. In order to complete these tests, we observe survey responses over many years. Showing that the particular explanation with social mechanism applies, after controlling for contextually specific factors, further strengthens the explanatory claims.

Statistical analyses apply demanding tests for the validity of empirical accounts.¹⁹ Moving beyond regression analyses, recent advances suggest that no area is immune to these methods.²⁰ They may be applied to probabilistic outcomes; to time-related processes characterized by autocorrelation, multilevel analyses that link individuals, families, neighborhoods, regions, and nations; and to endogenous relationships in which actors are expected to influence each other. They offer results that are easily transformed into probability statements. Here, it is possible to specify the impact of an explanatory variable (or a set of these factors) on the outcome while holding the other variables constant. Tables and graphs allow for easily interpreted results. The result tests for the internal validity of the empirical analysis against the power of alternative social mechanisms, relevant case-specific data, and against the possibility of random effects. Combining these precise results with social mechanisms provides the appropriate language for explanation in comparative politics.

Following these principles, we analyze data drawn from two extensive, complex, and representative national surveys, the German Socio-Economic Panel Study and the British Household Panel Survey. These surveys interview all persons over the age of 15 in a very large cross section and panel samples of households in each country. We explore 17 years of the German data and 11 years of the British responses. Each offers internally valid measures of the elements of partisanship. Taken together, they provide us with the most extensive collection of responses on partisanship while offering a treasure trove of information on the health, occupational life, and residential life of Germans and Britons, to name but a few of the surveys' themes.

¹⁹ Given the unfortunate battles between those who prefer quantitative and qualitative research, perhaps I should add that I do not suppose that statistical models have no flaws. Gary Cox underlines the relevant point: “. . . [T]he question about both statistics and game theory is not whether or not they are immune from abuse or misuse. They aren't. The real question is whether there are any systematic ways of thinking through correlations about beliefs and belief-desire stories, respectively, that are superior” (2004: 182). One must always defend one's methods.

²⁰ There is no reason to accept the claim that the probabilistic nature of political phenomena takes their analysis beyond the world of statistics. After all, stochastic phenomena are “those whose behavior can be analyzed statistically but not predicted precisely (Braumoeller 2006: 270, citing the *Oxford English Dictionary*, 2nd ed.). Clark, Gilligan, and Golder (2006) also apply statistical models to an area that has generally been the preserve of qualitative research: asymmetric hypotheses.

The data permit us to apply statistical models to the endogenous processes that account for the choices on partisanship and voting. For example, we use two stage probit models for the reciprocal relationship between the choices of wives and husbands. Controlling for the effects of more distant social factors, the results detail that partisan agreement is autoregressive on shared social class and religion but is not reduced to these more distant social contexts. Our models also control for election year and the secular decline of partisanship in these countries, which we expect to influence the elements of partisanship and vote. Linear probability models allow us to do the same for all members of the household. In a similar fashion, we apply other two-stage models to analyze together partisan support and choice and turnout and vote choice. Other models help us account for both partisan preference and consistency while controlling for alternative explanations.

Our findings place wives/mothers at the center of households: Their partisan decisions influence the partisanship of everyone else, and the others affect them; neither statement consistently applies to husbands/fathers or children. Consider, for example, how household members in combination affect each other's partisan choice. Wives/mothers are central. When both husband and child name Party A, the wife/mother's predicted probability of choice varies from two to four times above the mean. Husbands/fathers are different; the effect of children does not usually add to the wives'/mothers' impact. In three cases, mothers are so important to the children's choice that adding the father's concurrence adds to the probability of choice by less than .10. Note also that where two household members do not support the same party, the probability of partisan preference for the other member drops well below the mean (ranging from less than .05 of the mean for children and the Conservatives to .67 for husbands/fathers and the two German parties). For mothers/wives and children, the absence of concurring support from the others in the household makes it virtually certain that they will not name Party A. Households have cumulative effects, even as individual members influence each other's partisan preference.

The book presents an explanation of choices related to partisanship and voting in Germany and Britain. We apply social mechanisms that view social learning as an endogenous social process. We test them with a series of different statistical models that also control for alternative explanations. Our book's conclusion extends the explanation to other established democracies by including variations in the characteristics of the political parties in a verbal argument. We consider it to be a successful explanation.²¹

AN APPLICATION TO POLITICAL VIOLENCE

Research on political violence needs to clear many hurdles before it can claim explanations that demand acceptance. Here, poorly defended social mechanisms offer pitfalls to those who would apply instrumental mechanisms; relatively few

²¹ Reviewers will help to establish or reject our claim; scholarship too is an interactive social process.

use those drawn from social processes, and many display serious errors of concept, measure, and research method. The mean study is not able to demonstrate its explanatory claims.

Note some difficulties with the application of the social mechanism of instrumentalist choice in these illustrations. Political violence would seem to be an instrumental choice in which those who direct action – the leaders – benefit more than those who act – those who do the actual fighting. Political violence occurs even though the fighters, who bear all the direct costs, act in a circumstance in which potential injury (literally in this case) must certainly outweigh expected gains. Why then do the fighters fight? What accounts for the initial attack when there is an obvious clash between the interests of leaders and followers? Instrumentalist analyses of political violence need to account for the acts that compose political violence – who attacks whom, how, and how the conflict unfolds.

No matter the successes of Kalyvas's theoretical and empirical efforts, they too face difficulties. At the most fundamental level, it is difficult to see how resistance to Nazi rule belongs to the category of civil war, defined as "*armed combat within the boundaries of a recognized sovereign entity between parties subject to a common authority at the outset of the hostilities*" (Kalyvas 2006: 5; see also p. 17; emphasis in the original). On what grounds is Nazi rule considered a "recognized sovereign entity"?²² The analysis also does not distinguish between monopoly, which may derive from overwhelming force, and the legitimate monopoly on violence, which sustains legitimate authority (to return to Max Weber's critical distinction in "Politics as a Vocation" [Gerth and Mills 1958: 82], and see Migdal's chapter in this volume). Conceptual clarity requires theoretical support to cut at "nature's joints."

The studies also vary in the rigor of the empirical analyses. Critical dependent variables, such as political violence, civilians, insurgents, and ethnic riots, lack conceptual clarity and agreed-upon measures (see also Brass 1997; Fearon and Laitin 2000; McAdam, Tarrow, and Tilly 2001, 2007). They lack too the theoretical bases that underpin agreement on the meaning of measures of concepts. Because debates about guilt and innocence usually accompany this subject, using secondary sources for empirical evidence is particularly troubling. Not only is the analyst not in control of the data, but the sources too may be especially interested in justifying or condemning the events under examination.²³ Because the personal and the political overlap at the point of conflict, it is difficult to decide where the collective replaces the criminal. Note as well that in this realm

²² Also, the analysis notes but does not attend to the absence of resistance to Italian rule that preceded the arrival of the Germans. Furthermore, Kalyvas does not examine the relationship between the Soviet and Greek Communist parties during a period of Comintern dominance over that party and when Kalyvas notes that special Communist killing units initiated the violence (2006: 259).

²³ Contrast Petersen's (2001, 2002) attractive model of the relationship between social processes and Lithuanian attacks on Jews in the period before the Nazi occupation, with the constricted and highly partisan range of the evidence he examines.

of political research, survey questions also founder on doubts about the willingness and the ability of respondents to offer reliable answers. Other concerns apply to sources of aggregate data – government, police, and media reports – that do not sustain the internal validity of the descriptions (but see Wilkinson 2004 and Eck and Hultman 2007 for studies that offer strong tests for the validity of their data). Furthermore, because relatively few people engage in acts of violence (see Lichbach’s 5 percent rule in Lichbach 1995b) and because incidences of violence are uncommon (see, for example, Eck and Hultman 2007; Varshney 2002; Wilkinson 2004), rare events analysis or models that apply to data with many items scored as zero are the appropriate models, not the usual techniques of ordinary least squares regressions. Recall too that analyses of deaths in riots need to be part of models in which the analysis of riots is the model’s first step. Clearly, better data and the application of appropriate statistical tests will improve the success of explanations of political violence. By themselves, improvements in empirical analysis will not suffice. Research and theorizing need to establish the relative strength of social mechanisms too. Improvements in the theoretical principles that account for the decision to engage in political violence are at least, if not more important.

SOCIAL MECHANISMS AND RIGOROUS EMPIRICAL ANALYSIS IN ALTERNATIVE MODES OF EXPLANATION IN COMPARATIVE POLITICS

The relative dearth of responses to my chapter in this book’s first edition does not imply that comparativists have ignored the subject of ontology, theory, and explanation. To the contrary, filling the pages of books and articles and adding their voices to discussions at conferences and seminars, many comparativists have translated these issues into questions of methods and research design. Applying the principles of rational choice theory to historical and contemporary political events, others have examined how best to relate theory and evidence. In turn, these discussions of theory, evidence, method, and research design have brought causal mechanisms – if not social mechanisms – into the language of explanation in comparative politics.

Evidence embedded in theories and specifically designed analyses – not observations of political processes by themselves – establish social mechanisms. Experiments in the field and in laboratories, agent-based modeling, abstract reasoning about ancient principles, and emendations to rational choice theory substantiate them. Establishing the expected explanatory power of a social mechanism is a separate analytic step, independent of the observations and their tests that test the fit with the political phenomena being studied.

This position pulls me away from those who use “causal process tracing” in order to discover causal mechanisms. This method involves gathering information that seeks to “make sense” of the observations (Munck 2004: 114; Tarrow 2004: 173.) These authors contrast verbal accounts with information taken from a dataset, an “observation in the sense of a row in a rectangular data set” (Collier, Brady, and Seawright 2004: 252), and they offer process tracing as an alternative

to regression analysis (see especially Brady 2004, but see King, Keohane, and Verba 1994: 85–87, where process tracing and case studies are not equated with qualitative research). Still, as best as I can tell, it is less a research technique than an admonition that causal accounts include factors known by well-informed observers of politics to influence the outcome being explained (see also Beck 2006). This is not as simple as it may seem. Without demanding and multiple checks on the internal validity of the descriptions (see especially Campbell and Ross 1968; Cook and Campbell 1979), one must hope that the experts do not confirm each other's mistakes (Hamilton 1996), find patterns where none may be present (Gilovich 1991), and hold to a naive view of the relationship between concept and evidence.²⁴

On the same grounds, I stand apart from King, Keohane, and Verba's (1994) equation of explanation with the demonstration of a causal effect, even as I accept their principle that statistical models help to demonstrate explanations. As this chapter indicates (and see also Rodden this volume), one-directional causal effects are a subset of all possible relationships that might be explained, and so explanations in comparative politics must do more than show simple causal impact. By extension, their admonition that analysis needs to demonstrate "conditional independence" applies only where there is a one-directional relationship between causal and outcome variables, not where endogeneity applies. Similarly, the warning about problems associated with endogeneity keeps research away from these frequently found – indeed, fundamental – relationships. In addition, like others who engage in causal process tracing, King, Keohane, and Verba display little interest in the theoretical status of the causal variables. Consider how they treat the book's primary example, the effects of the predictor, "the causal effect of incumbency status for a Democratic candidate for the U.S. House of Representatives," on the dependent variable, the "Democratic portion of the two-party vote for the House" (1994: 77). They gauge this effect by holding constant a series of other potential explanatory variables. "During this hypothetical change, we hold everything constant up to the moment of the Democratic party's nomination decision – the relative strength of the Democrats and Republicans in past elections in this district, the nature of the nomination process, the characteristics of the congressional district, and the economic and political climate at the time, etc." (1994: 78). No theory supports the items on this list of explanatory variables. Instead, the authors offer an open-ended (for that is the meaning of "etc.") list of explanatory variables that knowledgeable observers expect to influence the causal effect. The presentation ignores how social mechanisms help to solve the problems with causal accounts and unnecessarily equates the demonstration of causal effects with explanations.

²⁴ Process tracing assumes the presence of "smoking guns," phenomena of obvious political importance. Consider, however, that German reunification hardly affects variations in political interest and has no impact on partisanship among West Germans (Zuckerman, Dasović, and Fitzgerald 2007). Steel (2004) offers a philosopher's defense of process tracing in causal analysis, but this too relies on well-founded social mechanisms.

My position stands in a complex relationship with Historical Institutionalism (HI), an emendation of structural theory (for a principled statement see Katznelson 1997; Katznelson and Weingast 2005a, and his chapter in this volume; for applications see, for example, Bense 2005 and Katznelson 2005). Like these scholars, I replace Marx's theory and its simple and determined ontology with a complex and open-ended portrayal of political processes. Like these scholars too, I emphasize the social context of political choices. There are important differences. HI uses case-specific information gleaned from summaries and reinterpretations of historians' accounts, not theory-embedded evidence, to generate causal mechanisms. Secondary analysis of published work stands at the edge of empirical analysis, as it is impossible to evaluate the validity of each and all of the accounts, as well as the political scientists' criteria for source selection. It is not clear who controls the empirical evidence (for a classic statement on the tendency of scholars to accept well-known but false "truths," see Hamilton 1996). Furthermore, HI shies away from statistical analyses even when there are models that are appropriate to the evidence and to the principled position that political processes are stochastic.²⁵

My general emphasis on social mechanisms resembles the research of Rational Choice Institutionalists (RCIs). Levi's chapters in the book's first and second editions illustrate this mode of analysis (and see Bates, Greif, Levi, Rosenthal, and Weingast 1998, 2000; and see Elster 2000 for a detailed critique). Rather than subsume cases under "covering laws," they "seek to account for outcomes, by identifying and exploring the mechanisms that generate them" (Bates et al. 1998: 12). Identifying actors as single or collective persons, their explanations use the principles of classical rational choice – including the selfishness axiom and instrumentalist choice – as manifest in the strategic calculations codified in the theory of games to provide causal mechanisms. Relying on a mix of primary and secondary sources, the chapters "seek to understand the actors' preferences, their perceptions, their evaluations of alternatives, the information they possess, the expectations they form, the strategies they adopt, and the constraints that account for their actions. We then seek to piece together a story that accounts for the outcome of interest" (ibid.: 10). Game theory constrains the empirical claims. As I utilize the social logic of political choice, I apply an alternative theory to derive the explanatory mechanisms.

²⁵ HI's opposition to quantitative analysis may be fading. Consider the recent change in Katznelson's treatment of research that uses variables. Praising the work of Barrington Moore, Katznelson declares, "He did not slice and dice his cases into variables which themselves would be compared as if they were not enclosed and entwined inside cases of dense and distinctive complexity" (1997, 89). Writing with Weingast, Katznelson modifies this criticism: "HI views variable-centered views of the world skeptically when they imply relatively closed systems in which the causal torque of relationships between or among variables is assumed to stay relatively constant across a wide range of times and places" (Katznelson and Weingast 2005: 15). It would seem that the problem is not with "slicing and dicing," but with how scholars use variables and their measures.

Theory and method are not alternative means to successful explanations. As I note throughout the chapter, pure observations are impossible; expectations guided by theories always guide observations. Gellner's principles link the two: The evidence must be beyond the control of the analyst. Theory by itself describes a virtual world, not the one in which we live and seek to explain. Empirical methods provide the means to bridge these worlds, and tests for internal and external validity help to strengthen these claims, even if no test or combination of tests ever suffices. The more that the observations examine explanatory claims that have high prior probabilities of success and the more demanding are the tests that they pass, the more reason there is to claim that empirical evidence supports the explanation.

CONCLUSIONS AND IMPLICATIONS

This chapter addresses explanation in comparative politics. Accepting the principles of methodological individualism, the analysis equates social mechanism with the general class of explanatory principles. Successful explanations join social mechanisms with rigorous empirical analyses. Advances in statistical modeling allow the application of these methods of detailing and defending the validity of the empirical claims to complex, reciprocal, and multilevel political phenomena, the stuff of comparative politics.

As I put the homily in practice, I highlight endogenous political influence, reviewing theoretical support and empirical evidence for this claim. Endogenous relationships characterize many, perhaps all, of social, economic, and political life, and I present sources from across the social sciences to support this claim. It follows that these should be central to successful explanations in political science. Conversely, where explanations examine only one-way causal relationships, the results are less likely to be successful. More generally, seeking to demonstrate the power of the explanation offered – and not just to offer a plausible account or a story in line with theoretical principles – the analysis insists on the need for both social mechanisms and rigorous empirical tests.

During the past decade, comparativists have come to agree – in practice and in preaching – that explanations require causal mechanisms. I do not challenge this consensus. Rather, I suggest that social mechanisms are the most appropriate form of these explanatory principles. Only they hypothesize what people know, believe, and value, and how these interact with their social, economic, and political lives. I suggest too that comparativists need to establish the expected explanatory power of the mechanisms before they apply them in research. Field and laboratory experiments, abstract reasoning, and agent-based models are the likely ways forward, not process tracing. Furthermore, explanations with social mechanisms cannot entail the truth of their claims; rigorous empirical analysis moves the analysis toward success. Statistical models provide a powerful way to test the validity of empirical claims – even complex, reciprocal, and probabilistic ones – and to explicate the explanatory processes.

An empirical science, comparative politics demands detailed, case-specific political knowledge and numerous strong tests of the validity of the empirical claims. There is no trade-off between these two forms of knowledge. The choice is not between emersion in the particulars of a case or the application of the appropriate statistical tools. Comparativists need to do both. In turn, because no one scholar is likely to possess both case-specific knowledge and the knowledge of how to apply statistical models, groups of comparativists should work together. Comparativists also need to learn from each other. Doing comparative politics, like choosing a political party, is a reciprocal and complex process.

Strong Theory, Complex History

Structure and Configuration in Comparative Politics Revisited

Ira Katznelson

This is a chapter of admiration, concern, and exhortation.

The admiration is for recent research and writing in comparative politics that are as impressive in their way as the landmark structural studies of vast historical change written in the 1960s and 1970s. Building on the intervening generation's diverse institutional studies, this scholarship has been oriented to important problems, notably including politicized identities, civil violence, sources of political legitimacy, and dimensions of inequality. Adopting a mainly pragmatic attitude about method, this work has shown a healthy disrespect both for overly stylized battles about paradigms and for persisting disciplinary tensions that pit qualitative against quantitative methods. Instead, its practitioners prefer to move back and forth from structural to agent-centered levels of analysis via the mediation of historical and rational choice institutional analysis.

There also has been a robust and unanticipated revival of writing based on "huge comparisons" of "big structures" and "large processes" (Tilly 1984). These studies exhibit substantive scope and methodological imagination to ask compelling questions about the creation and character of the modern world. Further, across many spheres in the social sciences, including areas that previously had been rather ahistorical, scholars have taken to heart the idea that social science and history cannot be constituted meaningfully without each other (Pierson and Skocpol 2002; Mahoney and Rueschemeyer 2003).

It would have been difficult to predict this efflorescence a decade ago. Large-scale historical social science seemed a spent force. Disciplinary pressures for more contained analyses and more cautious constructions of cases, an analytically powerful turn to microfoundations and deductive modeling characterized by a radical individualism and instrumental rationality, the crisis of academic as

I profited from a first set of suggestions by the editors and authors who gathered for a discussion of this volume at Bellagio in October 2006 (Samuel Barnes, Margaret Levi, Mark Lichbach, Howard Ross, Etel Solingen, and Alan Zuckerman). At the preliminary stage, I also received useful comments from Frances Hagopian, David Laitin, Evan Lieberman, Isabela Mares, and Kathleen Thelen. Close readings of the penultimate draft by colleagues Lichbach, Mares, and Zuckerman helped shape my revision. Donna Desilus provided research assistance.

well as political Marxism, and stylized battles about ontology and epistemology had all contributed to a marked substantive and conceptual retrenchment even in the work of the qualitative and historical successors to the grand tradition (Katznelson 1997). The unforeseen and welcome countertrends projected by the new range of writing in comparative politics thus invite critical appreciation.

The concern has two aspects. Existing intellectual energy might dissipate unless it is integrated by substantive research programs that can advance the field's level of ambition and help pull together its tightly targeted studies, making sums greater than the parts. For all its achievements, the new comparative politics is extraordinarily heterogeneous in subjects and questions, and risks stopping short of its potential unless it harnesses this diversity to endeavors comparable in extent to the quest to understand the origins and character of the big structures of modernity – including capitalism, civil society, and nation-states as both domestic and international structures – that concerned writing by scholars two generations back. Absent equivalent animating agendas, the magnitude of the challenges to which comparative politics can respond will be reduced. Without such projects, thematic literatures threaten to remain confined within specialized conversations, and possibilities for integrating findings across a range of discoveries are likely to stay artificially abridged. The force of recent scholarship might ebb and scatter.

A second worry concerns content and focus. By contrast to the centrality traditionally accorded the West by students of comparative politics, the more recent thrust has widened the field's geographic and thematic scope while presenting a very uneven focus on political regimes. There is much to appreciate in the systematic extension of comparative politics to literally take in the whole globe, consider a wide range of types of authoritarian and democratic governments as well as nonstate actors, and treat forms of global relations that often confound liberal-democratic states in the West, especially the United States. This decentering, however, risks eliding, perhaps even mystifying, hierarchies of global economic, military, and cultural power, and it chances downplaying the normative appeal and contested qualities of the liberal political tradition, the centuries'-old complex cluster of ideas and institutions that currently is dominant.

The exhortation proposes a close focus on the history, character, and conundrums of political liberalism. Such an assessment, addressing pressing issues and drawing on theoretical, empirical, and ethical resources, could advance the goals of ambition and integration by joining together various strands of work on markets, culture, civil society, democracy, mass participation, the state, and the international order. To be sure, there are limits to any programmatic appeal. Telling matters much less than showing. But in fact there is quite a lot of "doing" about liberalism within political science but not, disappointingly, with the equal, active, or self-conscious participation of comparative politics. This imbalance is costly, not just for the subfield, but for political studies and substantive understanding more generally.

Without making any claim that my suggestions constitute a privileged or exclusive way forward, I argue that by moving the variegated, complex, and challenging features of liberalism to the front and center, comparative politics

can probe significant structures, policies, ideas, and actions that are presently shaping the contours and prospects of political life. Should comparative politics take the lead in studies of liberalism, it could release the subject from the confined limits that characterize how the other areas of political science study the history and analysis of liberal ideas and ideologies, treat the status of liberalism in American political development, or consider the powers liberalism exerts in international relations. Each of these emphases is important but insufficient. A more holistic and wide-ranging consideration of liberalism can be convened by comparative politics because of the spatial, temporal, and methodological range it possesses. Moreover, such a project can demonstrate the continuing promise of the kind of configurational analysis deployed by the great studies of modernity's origins, an approach that combines strong theory with an unapologetic respect for history's variety.

I

Those studies by Perry Anderson, Reinhard Bendix, Barrington Moore, Charles Tilly, Immanuel Wallerstein, and other scholars sought to bring theory to bear on history in order to account for the decisive appearance of the great differentiated structures of Western modernity – a capitalism that was global, commercial, and incipiently industrial; states that claimed exclusive sovereign capacity to rule particular territories and specific populations with an ensemble of distinctive institutions legitimated by coherent normative stories; and a new international order dominated by the growth of technological, political, economic, and ideological capacities within Europe (Anderson 1974a, 1974b; Bendix 1964, 1978; Moore 1966; Tilly 1975; Wallerstein 1974). Intersecting logical with actual historical time, that scholarship emphasized how the structural constitution of reality coalesces to produce arrangements that decisively alter the probabilities of human action.

Although these studies strongly influenced students of comparative politics, they were not written in the main by political scientists, but primarily by professional sociologists. Within political science, an effort to join the behavioral revolution was more prominent. It was spearheaded by the Committee on Comparative Politics that had been called into being in 1953 at the behest of the Social Science Research Council's (SSRC) Committee on Political Behavior. Its research program promoted four goals. First was an expansion of the scope of analysis from formal institutions to political action outside the state, to what actors and associations actually do within political systems. Second was the effort to borrow and develop positive theory from elsewhere in the social sciences, including systems analysis and structural functionalism, to guide case research, which previously had not been sufficiently systematic or analytic, and thus make genuine comparative study possible. Third was the application of a Popperian scientific impulse based on systematic evidence, empirical testing, and the elaboration of potentially falsifiable hypotheses. Fourth was a geographic expansion well beyond the then-conventional study of Europe's "foreign governments" to non-Western and postcolonial

countries, treating the period's new states as loci to study patterns of interest, governance, and participation (Rustow 1957). Motivated by these aims, the SSRC Committee's book series, "Studies in Political Development," effectively reshaped the field's agenda (Pye 1963; La Palombara 1963; Ward and Rustow 1964; Coleman 1965; Pye and Verba 1965; La Palombara and Weiner 1966; Binder, Coleman, La Palombara, Pye, Verba, and Weiner 1971; Tilly 1975; Grew 1978).

The orientation and purposes of the scholars who led the tandem movement to write large-scale historical studies were different. Rather than study contemporary non-European circumstances, they stressed historical beginnings and the growth of a Europe-dominated globe. Rather than draw sustenance from modern empirical social science, as the SSRC Committee was doing, they stood on the shoulders of Western Marxists and leading late-nineteenth- and early-twentieth-century organizational, political, and social realists, including Max Weber, Otto Hintze, and Emile Durkheim. Rather than test competing propositions in the fashion of modern scientific inquiry, they produced plausible analytical histories to explain how the great structures of modern life had come into being and how, in turn, these arrangements had constitutively shaped such fundamental developments as forms of dictatorship and democracy, transformations to class relations and varieties of economy, and the growth of global integration.

When the next generation's "historical institutionalism" sought to advance this project, its members could not but be informed by the mainstream impulses that had been promoted effectively by the SSRC Committee and that had shaped their graduate training. As successor lines emerged, two daring books, Samuel Huntington's *Political Order in Changing Societies* (1969) and Theda Skocpol's *States and Social Revolutions* (1979), helped connect the period's different modes of work. Huntington placed contemporary political development in historical context. He sought to find keys to a threshold level of political stability in how institutions, especially, echoing a theme stressed by Giovanni Sartori, in how political parties, can shape, manage, canalize, and direct political participation (Sartori 1976). His institutionalism linked past to present and Europe and North America to a then still-emerging Third World. Skocpol's far-ranging but parsimonious focus on factors shaping modern revolutions likewise considered big swaths of historical time and geographic space, showing how to link Weberian organizational and state-focused emphases to Marxist modes of class analysis. Both sought to produce testable, variable-based propositions without foregoing the big questions that had been posed about states and society by Huntington's colleagues and Skocpol's teachers.

In bringing previously separate intellectual currents together, these books strongly influenced how historical institutionalists came to conduct their projects. Once again, the SSRC played an important galvanizing role. With Skocpol as a key lead player, in 1982 it sponsored a Committee on States and Social Structures¹ that sought to cumulate knowledge in a manner that could meet the

¹ The Committee, whose membership included Peter Evans, Albert Hirschman, Peter Katzenstein, Ira Katznelson, Stephen Krasner, Dietrich Rueschmeyer, Theda Skocpol, and Charles Tilly, was active from 1982 to 1989.

standards of method and evidence in the mainstream while holding fast all the while to the desire to explore substantive issues that matter by attending to the beginnings, development, concatenation, and causal effects of the modern state understood as an organizational constellation of institutions (Evans, Rueschemeyer, and Skocpol 1985; de Swaan 1988; Pierson and Skocpol 2002).

As the two main research programs of the 1960s began to cross paths, and as new influential hybrids began to attract younger scholars, a great deal of vibrant scholarship emerged under the heading of historical institutionalism, including rich and extensive attention, among other subjects, to public policy, welfare states, types of political economy, patterns of interest representation, and movements for social change (Berger 1981; Hall 1986; Esping-Andersen 1990; Thelen 1991; Immergut 1992; Steinmo 1993; Pierson 1994; Tarrow 1994; Marx 1998; Hall and Soskice 2001). Although an array of approaches to the study of politics came to be herded under this label, these shared, as key advocates observed, a family of traits. These included a penchant for substantive issues of wide concern, a wish to understand how institutional contexts and processes establish situations within which social action takes place, a desire to make time and its rhythms a constitutive feature of analysis, a search for prime movers of change, and a focus on the dynamics of state–society interactions (Pierson and Skocpol 2002; Sanders 2007).

Even with these achievements, some of us who identified with this intellectual project regretted how it was more confined temporally and topically than what had come earlier. More diffuse thematically and analytically, it also attended less to extraordinary moments of transformation and more to normal politics. The crisis Marxism was facing at the time, both in scholarship and in politics, was an important source of these reductions. Earlier structural work had rested on that tradition's understandings of structure, agency, and history's pathways. With a loss of confidence in such foundations, historical institutionalists tried, with mixed success, to replace a Marxist materialism with a more static and cross-sectional organizational materialism. They continued to tilt in the direction of structural causation, but turned to more conventional case comparative research based on the construction of comparable and divergent cases, relying heavily on John Stuart Mill's methods of similarity and difference (Skocpol and Somers 1980).

Writing a decade ago, I pushed back. While recognizing the gains that had been made by historical institutionalism's studies, I prodded macrohistorical scholarship to reclaim its place as a central research program. I sought to defend and advance not only the scale of earlier writing, but also its configurative approach to causality that, by way of strong theory, constructed cases as analytically motored narratives. Adopting an approach that treats actors as constituted by the structures within which they are located (rather than focus primarily on how such structures are created by goal-seeking individuals), that work had utilized theory, primarily but not exclusively drawn from the Marxist tradition, to construct cases in ways that made them susceptible to comparison without erasing their particularity, thus creating ways to think about the relationships among instances across both time and place (Clark 1998).

I identified that scholarship's configurational approach to layered causation with three pioneering books: Alexis de Tocqueville's *Democracy in America*, the great work that probed proper name history with theory about a small number of fundamental relationships – equality and liberty, religion and the secular public sphere, citizens and subalterns, institutional change under democratic conditions; John Stuart Mill's neglected proposals in *A System of Logic* about concomitant variation, showing how variables are joined together distinctively within and across particular historical instances; and Max Weber's guidelines for historical configuration in his *Methodology of the Social Sciences* that called for the identification of constellations of causes in order to analyze what each “unique configuration produces, since it is these individual configurations which are significant for us” (Tocqueville 1966; Mill 1852; Weber 1979: 72; emphasis in the original). Approaching causality this way, Moore, Anderson, and their colleagues had sought to reveal and analyze a concatenation of elements, selected both by historical learning and by a disciplined theoretical imagination.

Wagering on key factors at specific moments, their realist and concrete rather than nominalist and abstract approach aimed to discern a “sweet spot” located in the zone between high abstraction and particular specification. They were willing to run various risks regarding selection bias and were prepared not to dodge the truth that “the motor of history is endogeneity” (Przeworski 2007: 168). They understood that efforts to too rigorously manage or eliminate these problems by adjustments to the design of research can unduly inhibit and confine empirical inquiry, and thus limit an understanding of the complex outlier cases that are often the most challenging and interesting of all (Collier and Mahoney 1996).

In promoting a more ambitious comparative politics, I drew attention to vexing questions that concern the relationship of history to analytical political science, to post-Marxist investigations of structure, to the status of the state as a distinctive institutional site, and to microfoundations for structural analysis (Katznelson 1997: 94–97, 101). I also counseled attention to Albert Hirschman's plea for a substantive and methodological oscillation of perspectives when tackling big questions like competing interpretations of the character of market societies, and to Ralf Dahrendorf's demonstration that it is possible to hold on to the gains made by Marxist approaches to structural change and conflict without falling into traps of teleology, reductionism, and the identification of too narrow a set of historical factors (Dahrendorf 1959; Hirschman 1982a; Katznelson 1997: 100–102).

II

What are the prospects today for the revival I promoted? How might it best be advanced?

Looking back, a key element of advice was missing. I did not distinguish a range of pressing substantive issues on the scale of the earlier scholarship I so admire that were not being, indeed could not be, addressed within the bounds of existing scholarly trends. Nor did I anticipate the appearance of writing that

would begin to step outside those bounds and begin to reverse the retrenchment I lamented, including both a rich array of problem-focused writing that exhibits little respect for traditional divisions within comparative politics and the start of a return to big-scale structural scholarship. Any attempt to build a structural and configurative research program today will have to take this heartening enlargement into account, but current favorable conditions will not be fully exploited, I argue after a review of these intellectual developments, without being harnessed to a multifaceted and challenging research program.

Combining normative purpose with a diverse range of instruments, including deductive modeling, large-N quantitative methods, archival research, and extensive ethnography, the core of the new literature in comparative politics has been concerned less with what method to use than with identifying and probing a range of challenging issues: How do nationalism, religion, ethnic markers, and language entwine to organize human choice about politicized identities? Why do many large-scale projects that states promote to enhance human life often prove disappointing, or worse? When do party competition and state-building produce the private appropriation of public assets? Under what political conditions does the development of modern states advance the prospects of economic development, effective governance, political participation, and egalitarian patterns of distribution? How does the character and arrangement of institutions – including political parties, legislatures, business firms, and unions – and the relationships among them, protect or threaten democracy, elicit consent, advance or retard citizen participation and mobilization, shape lawmaking and patterns of delegation, and thus how states govern (Laitin 1998; Chandra 2004; Varshney 2003; Petersen 2002; Scott 1998; Grzymala-Busse 2007; Waldner 1999; Evans 1995; Rueschemeyer, Stephens, and Stephens 1992; Levi 1997; Collier 1999; Lieberman 2003b; Stepan 2001; Murillo 2001; Mares 2006; Swenson 2002; Boix 1998; Huber and Shipan 2002; Frye 2000; Kreuzer 2001)?

This new scholarship pivots on institutional analysis but defies easy or fixed classification. Consider, as an example, the study of violence in civil wars primarily based on postwar Greece by Stathis Kalyvas. Motivated by such vexing puzzles as the high degree of variance in violence, the uncommon brutality of civil war, and the formation and transformation of allegiances and identities in the crucible of warfare, the book “is simultaneously conceptual and positive, theoretical and empirical” (Kalyvas 2006: 5). It elaborates a model of selective violence, subjects it to test based on the wide-ranging data gathered in field and archival work in Greece, and carefully examines the potential portability of its approach to other times, places, events, and circumstances. And it moves fluidly by way of institutional mediations from macro- to microanalysis, from large-scale structures to intimate life.

In the systematic focus of the recent decade’s outpouring of books and articles on the content and implications of modern politics, most have moved well past the inductive variable-centered strategies that both rational choice and historical institutionalist scholars wished earlier to transcend. They refuse to choose between positive and normative orientations as they illuminate the character of

citizenship, the capacities of different types of regimes, distributive politics, war, race relations, and ethnic conflict, violence, and other vital subjects (Laitin 1998; Lieberman 2003b; Iversen 1999, 2005; Mares 2003, 2006; Mann 2005; Yashar 2005). There also has been growing methodological sophistication regarding how to combine variable-centered approaches with case analyses that build on structural and configurational models (Gerring 2004; Lieberman 2001a; Mahoney 2003; Lieberman 2005).

These intellectual developments, moreover, have been marked by fresh treatments of the origins and causal powers of institutions. Think in this respect of the stimulating books on Africa by Catherine Boone (2003) and Daniel Posner (2005), each of which, if differently, emplaces institutions at the heart of the analysis. Though distinguished by epistemological distinctiveness, they address overlapping subjects and deploy intersecting modes of investigation that explore the strategic logic characterizing particular situations, events, and outcomes.

In the early days of historical institutionalism, institutions were considered primarily as a preferred independent variable. More recently, institutional change itself has become a key object of analysis for both historical and rational choice institutionalists, a shift that notably characterizes Kathleen Thelen's *How Institutions Evolve: The Political Economy of Skills in Germany, Britain, the United States, and Japan* (2004). Like earlier work by historical institutionalists, this book is skeptical about the comparative statics approach characteristic of rational choice efforts to account for how institutions solve problems like cycling and overcome barriers to collective action. But Thelen also pushes beyond the various path-dependency approaches that have been very influential within historical institutionalism, works that focus on the closure of alternatives over time once a fateful initial step has been taken based on such mechanisms as positive feedback and increasing returns (Pierson 2000). She observes persuasively that just as rational choice work has been stronger on purposes than origins, scholarship based on path-dependent temporalities has been stronger on persistence than change. Instead, Thelen makes institutional development a central theme by looking systematically at periodic political realignments and negotiation in a way that invites comparative analysis over long periods spanning many decades, and also to more systematic work on strategic interaction. Indeed, she self-consciously identifies with Avner Greif and David Laitin's call to move beyond models of change that draw too sharp a line between stability and innovation but understand that many key sources of change are endogenous (Greif and Laitin 2004). Further, by stressing the contested nature of institutional development in contradistinction to path-dependent "lock-in" models, her work finds an enhanced place for the analysis of agency and choice.

The "play" that has advanced comparative politics, in short, has been less interested in adjudicating between the different institutionalisms than in drawing from a range of analytical traditions to answer tough and meaningful questions. It has been characterized by methodological range, playful combinations of research traditions, and a wish to locate the adjudication of hard questions in a zone where historical institutionalism and rational choice either

meet or overlap, a place where the expanse of questions, research, evidence, and arguments can be extended, but not jealously or exclusively, as in a great rivalry.

Overall, the distinction between large-scale historical social science and the more contained historical institutionalism has become too simple, as lines distinguishing such work and successor historical institutionalist scholarship from rational choice institutionalism have become more opaque. Some of the most important work on how economic and social structures determine comparative political outcomes, for example, is now coming from genres that once seemed quite distant from such an enterprise. In this vein, Adam Przeworski, Michael Alvarez, Jose Antonio Cheibub, and Fernando Limongi's large-N study of the relationship between economic development and democracy over the course of four decades suggests important lessons about the difference between origins and persistence (Przeworski, Alvarez, Cheibub, and Limongi 2000). Working with a large-N analysis of democratic and authoritarian outcomes over a century-long span starting in 1950 in tandem with case studies of Switzerland and the United States, Charles Boix deploys game theory and class analysis to understand political transitions and to model "the occurrence of democracies, right-wing authoritarian regimes and revolutions leading to civil war and communist or left-wing dictatorships" (Boix 2003: 2–3). Margaret Levi's work on military service combines an important extension of rational choice theory into the normative zone of "contingent consent" and "contingent refusal" with a wide temporal and empirical range of case materials on France, Britain, the United States, Canada, New Zealand, and Australia (Levi 1997). Across the arc of this scholarship, rigor is valued, but only when it advances our ability to ask serious questions and produce findings that illuminate questions about the kinds of central outcomes and values that concern profound issues, the kinds of questions about collective life, social decisions, legitimacy, and justice that have been at the center of systematic ethical and political thought for many centuries.

More and more researchers, in short, care rather less for old battles than wish to productively exploit the pluralism of institutional analysis, discern points of intersection, and combine qualitative, quantitative, and analytical approaches of various kinds that are informed by a rich knowledge of time and place in order to ask such questions and provide realistic, empirically grounded, accounts of political possibility (Laitin 2002; Katznelson and Weingast 2005). In so doing, they have privileged an understanding of situations, utilizing the various means social scientists have at their command to willfully simplify social reality by penetrating complexity in order to probe more deeply the character and particularity of vexing problems and outcomes. The gains to knowledge, especially gains to our understanding of non-Western politics, economics, and society, have been substantial.

The founders of the SSRC Committee on Comparative Politics more than a half-century ago have reason to smile, in part because the realization of some of their most cherished ambitions has been pursued by the deployment of a much more diverse toolkit than they then could have anticipated. But so, too, do the old-time analysts of large structures and comparisons who, standing on the

shoulders of Marx and Weber, did not shy away from the biggest questions about Western political modernity and believed that social science gives humanity a chance at social control in a perilous world. They surely would welcome evidence demonstrating how their grandchildren, as it were, have been using a hybrid of approaches to investigate more tightly many aspects of just such fundamental issues.

III

They also might be pleased to witness the second unanticipated trend, the recent revival of big-scale studies. Like their own work, a new burst of scholarship has sought to connect analytical thought and empirical drive to normative goals. There has been renewed attention to questions of the same scale and significance as the prior work. In the past half-decade, important books have asked why, in the late medieval period, Western trading capitalism displaced and supplanted the economic, technical, and scientific leadership of the Islamic world (Greif 2006); discussed when elite control of government is replaced by democratic rule and when dictatorships stabilize (Boix 2003; Acemoglu and Robinson 2006); probed how similar and how different are the strategies rulers deploy in different epochs and civilizations to expand territory at the cost of large-scale warfare (Hui 2005); and considered the deep historical sources that have affected the construction of alternative types of modern states in the developing world, states with radically different implications for economic development (Kohli 2004).

One of the most striking features of this renewal of large-scale historical and comparative social science with the ambition to understand the birth of capitalism, the origins of different types of regime, the sources and range of inequality, the patterning of global power, and other such grand subjects is its epistemological range. Some of the work follows prior established lines of qualitative comparative and historical inquiry while also imaginatively expanding its reach. In this vein, Atul Kohli's *State-Directed Development: Political Power and Industrialization in the Global Periphery* (2004), a comparative analysis of the impact of state intervention in Korea, Brazil, Nigeria, and India, combines a long view about the origins of diverse patterns of political economy with a contemporary institutional analysis to account for the wide-ranging variation in outcomes among these countries. Victoria Tin-Bor Hui's *War and State Formation in Ancient China and Early Modern Europe* (2005) inventively utilizes the history of these two state systems as counterfactuals for each other despite their spatial, temporal, and cultural discrepancies, a divergence she effectively takes into account in a work located at the intersection of international relations realism and comparative historical social science. Crossing the borders of time and place, James Scott's *Seeing Like a State* (1998) embraces themes important to Michel Foucault's work on power and governmentality, showing how attention to that tradition of scholarship can engage with themes that concern the character of modern states and their transactions with societies and economies. Some ambitious work of this kind has been written by political theorists who engage

empirical research to better ask questions about the history and prospects of liberal democracy (Ober 1998; Mackie 2003; Elkin 2006; Rosanvallon 2006).

Arguably the most interesting facet of the revival of grand comparative-historical work has been the attempt by economists with a substantive historical bent, following pathways forged in work on structures and institutional transformations by Douglass North (1981, 1990), to specify micro-foundations in preferences and strategic interaction for macrohistorical change at times of great historical change. The two leading examples are Daron Acemoglu and James Robinson's *Economic Origins of Dictatorship and Democracy* (2006), a book that owes its subject and title to Barrington Moore's pathbreaking work of 1966, and Avner Greif's *Institutions and the Path to the Modern Economy: Lessons from Medieval Trade* (2006). Despite their broadly common provenance, common understanding that human welfare often is determined by the institutional foundations of politics and society, and shared interest in specifying the processes that bring about and shape the character of institutions, their ways of working diverge considerably regarding levels of abstraction, the utilization of evidence, and judgments about whether theory should be kept focused and parsimonious when anomalies appear, or whether such anomalies should trigger expansions of theory beyond classical assumptions. Thinking about this analytical range in the treatment of institutional dynamics can help clarify how this economy-centered initiative can contribute to future work.

Like Moore, Acemoglu and Robinson present what essentially is a structural theory, "a unified framework for understanding the creation and consolidation of democracy" (Acemoglou and Robinson 2006: xiii) over the course of half a millennium based on a relatively unadorned class-based model of inequality, in their case one that distinguishes prosperous elites who are afraid of democracy because it potentially threatens their income and wealth from poor nonelites who support democracy to gain more economic assets (a middle class also appears, wishing for controlled and limited democracy, a political in-between). Unlike Moore, they ground their work in economics, not social theory or political sociology, to discern and specify a logic of process that either increases or reduces democratic prospects, particularly wishing to understand the conditions under which inequality induces elites to share political power and nonelites to trust elite dispensations. They transform the question "Why is it that some countries are democracies, where there are regular and free elections and politicians are accountable to citizens, whereas other countries are not?" into puzzles about why elites would ever share power with others, and why nonelites ever would believe that what elites grant under pressure will not later be withdrawn (Acemoglou and Robinson 2006: xi, xii). To the first issue, they treat democracy as a concession aimed at stemming insurgent conflict by the majority. To the second, they conceptualize democracy as a set of credible commitments, an institutional solution marked by a durable redistribution of political capacity.

Based on traditional rational choice assumptions about economic motivations, incentives, and strategic behavior, they develop both static and dynamic models of democratization privileging conflict based on clashing class interests

to demonstrate how and why, under particular conditions of duress, inequality, and mobilization, elites judge the price of democracy to be lower than the price of repression and dictatorship. Democracy happens, they argue, when inequality is neither so great as to make the rich unwilling to grant concessions nor so small as to make the poor uninterested in mounting challenges, but rather when inequality falls in a middle zone that generates unrest and conflict from below and a logic of situation from above in which conceding democracy is less costly than resisting institutional change.

Greif tackles and links two equally fundamental historical puzzles, the sources of market development in the late medieval period spanning the eleventh to fourteenth centuries and a history of divergence between the Islamic world, whose economy and commercial integration were quite advanced, and the European, which, at the time, was experiencing a major commercial revolution. He further wishes to understand what North and Thomas's classic book, focusing on the role of government, had called the "rise of the Western world" (1973). Greif asks why, despite the relative scientific, technological, and economic advantages possessed by the Mediterranean Muslim world, the manner in which both economic systems proceeded produced a global inversion in which Europe supplanted its competitor to achieve economic (and territorial) domination of the globe.

His account is deeply institutional, pivoting on contracts, economic associations, and trust. In the eleventh century, both economic systems were characterized by comparable features. But by his period's end, European countries possessed permeable states and mechanisms of political representation, autonomous associations that transcended kinship in the economic sphere through the development of corporate guilds, and more secure, comprehensive, and enforceable forms of contract that the Islamic world did not. Generating trust, he argues, this institutional set, temporally preceding the role of the state stressed by North and Thomas, provided foundations for Europe's robust economic expansion by inducing widening networks of anonymous market exchange and risk-taking entrepreneurship.

Each bold and significant, these books have radically different implications for how to conduct comparative and historical social science. *Economic Origins* is an extended and multifaceted essay in "economy-based" deductive reason "in the sense that we stress individual economic incentives as determining political attitudes, and we assume people behave strategically in the sense of game theory" (Acemoglu and Robinson 2006: xii). It stylizes possible pathways and outcomes and builds a dynamic model of a repeated game that maps onto the volume's more "simple game of promises." Acemoglu and Robinson clearly know a great deal of economic and political history, and deploy examples drawn from England, Argentina, Singapore, and South Africa to illustrate their arguments and demonstrate the empirical plausibility of their models. But this is not an empirical or, despite its time horizons, a historical work. Rather, it is a book that fashions a rigorous theory of institutional change and development. The theory, though, is not tested with thick historical evidence. Actors remain stylized, even

abstract cartoons. Proper names do not matter. Nor for that matter is the book deeply innovative in theory, as it is content to adopt and adapt the genre's standard simplified assumptions and descriptions of actors, preferences, economies, and regimes.

Though at least equally at home in economic theory, Greif works very differently both as a historical social scientist and as a theorist of institutions. He starts with a strong desire to link history and analysis tightly, all the while aware of the pitfalls. "The book," he writes in language that might well have been penned by the early generation of large-scale comparativists, "aspires to do justice to historical particularities and processes," yet "it also recognizes that a purely historical narrative risks being ad hoc and devoid of general insights." And though "relying on explicit theory, using analytical models, and putting conjectures at the risk of being empirically refuted," he cautions, in terms that might have been used by historical institutionalist critics, that "general theory often fails to account for historical particularities; the use of models is restricted by underlying mathematical techniques; and historically specific conjectures often cannot be evaluated using standard statistical models" (Greif 2006: xiv).

Working with the desire to show how historical and social scientific approaches are "complements to each other rather than substitutes," Greif makes two major contributions to "context-specific, theoretically informed" social science (Greif 2006: xvi). The first, as noted, is that of filling in key blanks to our understanding of the origins of central features of the modern world by elaborating a deeply institutional account of Western economic development. He traces in great detail, using a wide range of historical sources and secondary materials, how a particular institutional set developed that facilitated long-distance trade without making any of the usual soft functionalist assumptions that we have secured the institutions we need.

North and Thomas had argued that the rise of the West was due to the creation of efficient economic institutions, including secure property rights, as progenitors of incentives for economic initiative. But how do such institutions come about? They used standard and rather opaque language, arguing that "new institutional arrangements will not be set up unless the private benefits of their creation promise to exceed the costs" (North and Thomas 1973: 6). But this will not do for Greif, as he insists that multiple equilibria are always possible. Going beyond North and Thomas's seminal idea of institutions as incentive structures, he tracks the highly contingent process by which the West actually achieved the institutional set that built the networks and trust required for long-distance trade based on impersonal exchanges. In so doing, he also fills in blanks in the work of the historical economic sociologists of late medieval and early modern Europe, including the major studies on the origins of capitalism by Wallerstein and Anderson.

Greif also intends something more: a guide to how to think about and study the sources, character, and effects of institutions with tools at the junction of history and social science. He is unsatisfied with how economists and many rational choice scholars treat institutions aseptically, as rules in extensive form games that either are politically imposed or develop as optimal responses by

agents. Institutions are always constellations that combine rules with culturally, geographically, and historically specific norms, values, beliefs, formal organizations, and social practices. Institutions are complex sites linking structure to action. An institution is not a monolith, but “a system of social factors [that] conjointly generate a regularity of behavior” (Greif 2006: 30). Institutions strongly affect preferences and probabilities, and thus shape the regularity of social behavior distinctively at different moments and locations. Formal rules only motivate conditionally, and the force of an institution is dependent on how the larger milieu affects human understanding and motivation. Institutions organize the character of transactions, the process of linking and transferring from a given social unit to another. Institutions generate behavior in a setting or pattern of transaction. The key site of institutional analysis thus is “intertransactional linkages – economic, coercive, social, and normative” (Greif 2006: 46–47, 147). Working in this way, as Edgar Kiser observes, Greif’s “basic unit of analysis is thus not the game, but the transaction” (Kiser 2007: 43).

In Greif’s approach, people act rationally and strategically but only within a probabilistic social and normative framework that affects their propensities. Asking whether “homo sociologicus is strategic,” he avers that preferences are not fixed givens, but come to be established inside institutions that impart instrumental and moral cues. They are embedded within normatively and culturally distinctive institutional environments that themselves are historical products (Greif 2006: 421–427). These institutions, he maintains, are inherently indeterminate, specific to context, and rich with unobservable as well as observable components. Deduction (an inference in which the conclusion about particulars follows necessarily from general or universal premises), he thus insists, like that performed by Acemoglu and Robinson, is limited by an inability to project institutional structures from the exogenous characteristics of situations because a “multiplicity of equilibria” are “likely to prevail in exactly the kinds of situations of interest to institutional analysis.” There is, moreover, “no theory to indicate which game is relevant to a given transaction at a given time and place. Similar circumstances do not “lead to the same institution in all historical episodes” (Greif 2006: 353). But pure induction will not do either, lest we lapse into just-so stories. Instead, his guidance for the conduct of “interactive, context-specific analysis” puts forward one fruitful way to navigate the inevitable challenges that come when big historical ambitions meet the purposes of social science (Greif 2006: 366–376).

It is, of course, not the only way to work. There is no consensus about how to conduct large-scale social science. Usefully, there has been a proliferation of efforts to propose how social studies with considerable sweep might best proceed and that help us think through how the elements in the triad of rationality, culture, and structure at once compete yet beckon to be combined. Consider two recent examples. In an effort to prod and support a post-Marxist qualitative-historical project, Charles Tilly and the political theorist Robert Goodin brought together some 40 colleagues to write about what they call “contextual political analysis,” the name they give to systematic accounts of large-scale

episodes in history. Looking for alternatives to game theory and inductive description, this collection explores how the resources of philosophy, psychology, and the study of ideas, culture, place, population, and technology can inform scholarship that takes context seriously. Like Greif, Tilly and Goodin prefer an ontology of “relational realism” – “the doctrine that transactions, interactions, social ties, and conversations constitute the central stuff of social life” – an approach they identify with Karl Marx, Max Weber, and Georg Simmel, as distinct from a methodological holism in which structures with sustaining logics are all-determining, and from a strong methodological individualism that begins with the intentionality of persons, one at a time. Like Greif, they also favor a mechanism-centered logic of explanation, distinct from accounts based on hyper-strong theory that seeks causal social laws at a high level of generalization or those that abjure the causal enterprise because they view history as “so complex, contingent, impenetrable, or particular as to defy explanation.” Instead, they opt for mechanism-based accounts, with mechanisms understood as forming “a delimited class of events that change relations among specified sets of elements in identical or closely similar ways over a variety of situations,” an approach that treats historical processes as “combinations or sequences of mechanisms” (Goodin and Tilly 2007: 10, 12, 15).

Another enterprising but quite different attempt to advance a resurgence of large-scale social science seeking to comprehend modernity is the 17-essay collection edited by the sociologists Julia Adams, Elisabeth Clemens, and Ann Orloff. In light of what they see as the shift from a first wave of work that was broad and audacious to a second wave that was characterized by a tighter framework of subjects and hypotheses, they advocate a third wave that further pursues recent “lines of convergence” by placing them in close contact with a postmodern impulse that wishes to “‘remake modernity,’ by which we mean critique and reconstruct the modernist categories that have informed historical sociology to date” (2005: 3, 7, 64). Walking “an analytic tightrope,” and deeply informed by the wish to better comprehend the cultural construction of human life, they advocate a deep conceptual reconsideration as a way to find tools that bridge the impulses of the first and second generations. They draw sustenance not only from the various institutionalist approaches, albeit with rather deep skepticism about rational choice, but also from postcolonial studies and feminism. Wishing, like other critical voices, to attend to issues that have only achieved a marginal place in comparative politics (Steinmetz 1999), they counsel more room for human agency, a democratization of the subjects of history to include the subaltern, and more attention to the microdynamics of power.

IV

For sure, this plethora of work stands side by side with narrower, less ambitious, more method-driven scholarship. Nor do the various strands of work over the course of the past decade constitute anything like a consensus about research paradigms. But that is one of their strengths, for we now possess a considerably

wider and deeper range of vantages from which to observe and study central puzzles about politics. And within that mix, structural and configurative approaches are exerting influence both among scholars who self-consciously identify with this tradition and among others who primarily look elsewhere for intellectual sustenance.

Still, there is a long way to go. Notwithstanding the various promising trends and impressive accomplishments, large-scale comparative historical analysis even now is not quite the driving force it once was. It has yet to overcome a lingering predicament of disenchantment and stalled development. So, it is important to ask how the considerable intellectual energy and widened range of assets in comparative politics can best be channeled to propel this tradition and, in that way, enhance political studies more broadly.

The promise of large-scale work in comparative politics will remain tantalizingly unrealized unless pressing and coherent substantive projects galvanize the field's recent gains. At the high point of the 1960s and 1970s, Moore, Anderson, and their colleagues possessed a coherent mission based on the drive to know in order to know how to act. Their focus on great branching moments, immense processes, grand structures, and powerful vectors of history was an attempt to find meaning and a place for more contemporary politics and possibilities. Most important, they utilized these means to grasp how the great structures characterizing modern life had been fashioned, and how these arrangements continued to constrain and shape choices about regime paths, violence and war, and vectors of inequality. The goals and plans for research that characterized their grand historical social science fired imaginations across disciplines and generations. In their pursuit of understanding, those scholars possessed a kind of confidence – a rare sort of confidence – that somehow it would be possible to artfully deploy tools of social theory and empirical analysis to discern guideposts for action in the here and now, or at least to help understand the character, sometimes the deeply disturbing character, of present-day politics and society.

The Western liberal tradition – a variegated and not constant family of ideas, concepts, ideals, and institutional arrangements for political life that first were elaborated in early modern Europe and fitfully developed both in a series of conflicts with numerous illiberal adversaries and through debate within the liberal camp – offers just such a subject. As a guardian against depredation, as a guide to decent politics, as a source of political imagination, as a progenitor of institutional design, as an organization of power, as a leading way to organize political life across much of the globe, and as the target of challenges from a wide variety of intellectual, religious, and political movements, liberalism presents historical and conceptual, normative and empirical challenges of the highest order. Referring not just to Western actors, the philosopher Raymond Geuss recently has observed how humankind currently faces “the odd situation” in which “we seem to have no realistic alternative to liberalism; that is, we know of no other approach to human society and politics that is at the same time as theoretically rich and comprehensive as liberalism and even

remotely as morally acceptable.” Yet, for reasons associated with how liberal regimes often fail to be consistent in their values, practices, or principled commitments, especially with regard to persons and countries that fall outside the ken of liberal membership, and because liberalism is often identified as an elitist force of cultural imposition and economic predation, it frequently is resisted as a guide to institutional arrangements and political action. As a result, Geuss observes, liberalism is presently confronted with “significant theoretical, moral, and political disaffection,” including concern that its key values and putative universalism might conceal, shield, and advance deep inequality and global hegemony (Geuss 2002: 320–321).

In such circumstances, understanding and coming to terms not just with liberalism’s moral appeal or with its successes, but with the full range of its character, content, deficits, vulnerabilities, and causal capacities, is urgent. Making sense of liberalism as a complex political site, not just normatively but also empirically, could hardly be more imperative. At least two master themes beckon – one as old as political thought, the other a centerpiece of the last half-century of political studies. A long-standing and venerable subject, at least since the time of Aristotle’s reflections, has been the question of types of political regime. In the West, some of the most fierce struggles from the eighteenth century to the present have pitted different forms of governance against each other, and in the twentieth century liberalism emerged as the only viable alternative to fascism and communism. Today these battles take other forms, as liberalism has become a target as well as a source of approbation. A related theme has been that of understanding political and policy variations, not limited to economic affairs, within the liberal tradition, including social democracy, Christian democracy, social liberalism, and what in the United States is called conservatism but elsewhere often is thought of as neoliberalism.

Thinking systematically about liberalism, moreover, provides an opportunity to connect theoretical inquiry and empirical research in a manner that links subjects and puzzles that usually are considered separately. It can generate fresh scholarship on the history and character of modern politics and society that reaches an older level of ambition without sacrificing, indeed by relying on, key moves and insights from more recent research and writing in comparative politics.

If this is a compelling agenda, the divisions in our discipline, especially that which separates comparative from American politics, but also from political theory and international relations, need to be bridged more frequently. There are some, but too few, promising signs. Recent treatments of race relations by Anthony Marx (1998) and Robert Lieberman (2005), for example, have prominently situated the United States within the ambit of comparative historical and policy analysis. Political theorists have begun to write empirically grounded reflections on liberalism, as in Uday Mehta’s *Liberalism and Empire* (1999). There are more and more calls like that of Helen Milner (1998) to cross the line that usually separates global from domestic studies. But these developments have not been animated by explicit and linked temporal, conceptual, historical, and normative purposes. Thus, they often seem disconnected.

Nonetheless, there is a sense in which liberalism has been the master topic of American political science since its founding as an organized professional community of scholars at the start of the twentieth century (Katznelson and Milner 2002a). The diverse elements in the discipline have been concerned, from the start, with how liberal polities operate, with when they succeed and when they are vulnerable to illiberal currents, with how they bond with other traditions, including democracy, and with how the scope of liberal citizenship is determined.

Yet, for students of comparative politics, liberalism has functioned much as Louis Hartz famously argued about its status in the United States as being so pervasive that, like the air we breathe, it is unnoticed (Hartz 1955). Over time, moreover, liberalism as an explicit topic and object of analysis has come to “belong” to other parts of political science, primarily to the subfield of political theory, both to its histories of ideas and to its analytical efforts to probe the tradition’s varieties and advance or critique various constellations of liberal doctrines and practices (Kalyvas and Katznelson 2008). The subject also has been the province of the American Politics subfield, where scholars have vigorously debated the status of Lockean formulations in what manifestly is a polity whose rules and constitutional arrangements are grounded in liberal conceptions of governance (Hartz 1955; Greenstone 1993; Smith 1997). Recently, International Relations also has turned frontally to liberalism, especially in revivals of Kantian themes about propensities to war and peace, in puzzles about ties connecting liberalism and imperialism, and in new theories of international affairs that illuminate how ties between states and their policies are connected to both their domestic and global political contexts (Doyle 1983a, 1983b, 1986; Russett 1993; Owen 1994, 2001; Latham 1997; Ray 1998; Morrow 2002; Hoffman 1995; Johnson 2003; Moravcsik 1997).

By contrast, students of comparative politics have participated more indirectly, in more limited fashion. Curiously, there has been something of an imbalance and a constrained agenda in the subfield’s treatment of the liberal tradition. Attempts to understand historical and contemporary variation in forms of liberalism in recent years have been most notable and vigorous in explorations of the “varieties of capitalism,” which can be read in part as studies of “varieties of liberalism” that have examined such matters as different political sources of comparative advantage, the role played by corporatist models, changes in economic institutions that coordinate the economy, and the patterning of cross-class alliances (Kitschelt, Lange, Marks, and Stephens 1999; Iversen, Pontusson, and Soskice 2000; Hall and Soskice 2001; Swenson 2002; Pontusson 2005; Streeck and Yamamura 2005). This has been valuable work. But like the earlier historical institutionalist thrust, it has focused heavily on variations to liberal policies in the realm of economic affairs. Thus, it serves both as a site of scholarship that cautions us about the constraints on how comparative political scientists have been writing about the liberal tradition and as a template signifying how they can conduct fruitful work on central transactions characteristic of liberal regimes (see Mahoney 2005; Sassen 2006).

Despite the limited role comparative politics has played to date in developing a coherent research program about the liberal tradition, the potential exists for the subfield to be *the* central place within political science for a nonprovincial, nonutopian, theoretically guided, global, historical, and empirical study of liberalism's matrix of rules, institutions, policies, and norms. In the main, political scientists in other subfields have focused historically and analytically on the liberal tradition in Europe and North America, thus with relatively narrow empirical horizons, if for justifiable reasons. But this limitation is now clearly obsolete. With an expanded scale, and with proper attention to issues of conceptualization, scope, comparability, measurement, disparities between form and content, and the porousness of liberal political boundaries, work in comparative politics can give form to the dispersed considerations of liberalism elsewhere in political science while drawing resources from history, sociology, economics, psychology, and anthropology.

Emplacing the complex subject of liberalism as a principal subject at the core of comparative political studies would require thinking about modern liberalism not simply as a collection of abstract concepts and guides including government by consent, political rights to guard against predatory states and rulers, and the values of toleration and pluralism, but as clusters of potential and actual rules, institutions, and policies that have been or could be deployed to manage central structural features of the modern world in a manner consistent with these basic normative principles.

Political liberalism originated amid religious bloodshed and immense structural change to find means to manage differentiation, secure order, and grapple with a hastening pace of global and local transformation. Above all, liberalism as a site of institutional invention has sought to apprehend and respond to the separation of the spheres of sovereignty, property, and society, just the subjects that the large-scale structural social science of the 1960s and 1970s had sought to understand. A concentration on liberalism is thus something of a natural successor, especially if we think of this zone of inquiry in terms of the quest to fashion normatively appealing means to govern how transactions between states and economies, between states and societies, and between states and other states are conducted.

Such a focus on rules, institutions, and policies of linkage between big structures can make connections not just among different types of knowledge but among subject areas, including constitutional design, the status of property and the regulation of various kinds of markets, the standing and responsibilities of citizens, the scope of statist as distinct from religious authority, the boundary between public and private, and the terms of international economic and geopolitical relations. The study of liberalism also points to questions of inclusion and membership – who gets to be a liberal citizen? – and issues of pluralism and toleration – which ideas and practices are admitted into the ken of legitimacy? – and to closely related tensions inherent in its philosophical anthropology that portrays eligible persons and groups as free, equal, and rational, and thus capable of deliberation and decision. Because liberalism, from the outset, has been

premised on the existence of sovereign and relatively stable, efficient modern states, its study also provides a vantage from which to reconsider issues of state formation and governance, including the span of liberalism's global ambitions, and the range of nonliberal norms and practices with which it might bond. Further, this subject invites particular emphasis on how, under a variety of conditions across time and place, private individuals and groups can make their preferences known via political representation inside the state, how governments manage such pressures, how policy coalitions form and act, and how these inherent features of liberal political life interact with the substantive content and results of lawmaking.

It is a fundamental and inherent feature of the liberal political tradition that individual and group actors and their preferences matter (Katznelson 2003b). They pursue their often conflicting goals in a differentiated world with conflicting interests, values, beliefs, and commitments. What Thelen, Greif, and other institutionalists help show us is how to discern the structural embeddedness of such preferences, how to think about the origins of institutional arrangements that define the character of situations and the probabilities for the creation and development of different types of preferences, how to focus on transactions as rule-governed units of analysis, and how to inquire about institutional change. What the problem-focused literature in comparative politics teaches is that the advantages of a possible research program on the liberal tradition must build on, incorporate, and extend the wide array of important subjects about global and domestic affairs that this work has been probing rigorously.

The study of liberalism in a historical and comparative mode also invites attention to the issues of temporality that historical institutionalists have brought to the fore at a middle level of theory within fairly well-defined scope conditions (Hacker 2004; Pierson 2004; Streeck and Thelen 2005; also see Kiser 1996). At the heart of this effort to deal with periodicity is the identification of critical junctures (Collier and Collier 1991). From within a research project on liberalism, it makes sense to treat such moments as those when each of the key sites of transaction – state–economy, state–society, state–state, and rules for membership – are simultaneously in flux, calling into question the capacity of liberalism both to restructure the status quo and to compete with other regime possibilities.

One such long moment deserving more systematic attention is an epoch in the history of humankind at least as momentous as the time when modern states and modern commercial and capitalist economies were fashioned – the decades that span either side of the Second World War. That time, characterized by exceptional transformations in the level of violence, the willingness to make other humans superfluous, a vast extension of the organizational scale and predatory capacity of state power, and the enlargement of regime types, witnessed both the death of older liberal ideas and faiths and attempts to fashion a new, less credulous liberalism based on fresh ideas in new institutions (Katznelson 2003a, 2007). Curiously, although there is an immense literature

written by historians dealing with this fateful era, it has, with exceptions (Luebbert 1991; Ertman 1998), elicited much less attention from students of comparative politics than either early modern Europe or the more recent past.

Systematic inquiry in this complex field about the history, present, and future of the liberal tradition makes it possible to connect otherwise diverse and unrelated subjects, link alternative types of causal accounts, and join “experience-distant” coherent analytical, historical, and empirical work to “experience-near” real-world challenges in ways that might help us learn not only about the past but also how to navigate among present tests and challenges. For just these reasons, the sheer range and variety of the recent decade’s literature in comparative politics stands as a challenge and as an opportunity. So too does the chance to think about its relationship to an older and esteemed body of work in comparative politics that highlighted key aspects of the sometimes difficult ties connecting liberal regimes and mass democracy (Lipset 1960; Eckstein 1966; Rokkan 1970; Dahl 1972, 1983).

The challenge, of course, is one of integrating these various older and newer strands into a thick, substantive research program on liberalism without reducing the field’s methodological and causal plurality. But it is just this possibility, especially in light of the enhanced global reach of comparative studies; their stress on relational ties between states and citizens, states and markets, states and other states; and their emphasis on identity, ethnicity, and political membership, that opens the prospect of a generous and pressing research program that is broadly oriented by a structural imagination and a configurative approach to causal analysis.

This is not the only way forward. And it is not without pitfalls. It risks seeming too Western, even too American, and not sufficiently comparative. Its focus on a type of regime may appear synonymous with satisfaction. It might also give the impression of being more normative than empirical. But none of these qualities is inherent if the work we perform is open, reflexive, and systematic.

As such work proceeds, we would do well to remember that social science best advances when it combines assertion with modesty. Research programs move ahead most productively when they push hard in preferred directions, but their power often depends on understanding that diverse and often dissimilar modes of knowing can illuminate different aspects of important questions. No single subject can suffice, and no mode of analysis ever can be entirely self-sufficient.

Reconsiderations of Rational Choice in Comparative and Historical Analysis

Margaret Levi

Intentional and rational actors generate collective outcomes and aggregate behavior that are often socially suboptimal and personally undesirable. This claim, supported by both logic and evidence, has long been the major contribution of rational choice scholarship. Rational choice has conclusively laid to rest the pluralist myth that individual interest automatically leads to collective action or that collective action necessarily produces a collective good. The guiding insight has a long heritage, dating back at least to Hobbes's *Leviathan* (1985 [1651]) and to Mandeville's *Fable of the Bees* (1924 [1714]). However, it rests on an impoverished model of human action and society. Even the great Adam Smith recognized this. Famous for *The Wealth of Nations*, in which he identifies the conditions under which following personal self-interest makes everyone (or almost everyone) better off, he also authored *The Theory of Moral Sentiments*, in which he states that values may trump interests.

Comparative and historical rational choice demands theoretically informed empirical investigations of variations in aggregate behavior and collective outcomes derived from models of strategically interacting individuals making reasoned choices subject to constraints. Rational choice analysis derives from but is decidedly not neoclassical economics. It has, at least in the work discussed in this chapter, a richer conception of reasoned choice, interactions, and constraints. Consequently, rationalists now can identify the conditions under which aggregations of rational individuals can produce optimal social outcomes. By recognizing differences in context and institutional arrangements, rationalists can elucidate why the same actors make different choices in different circumstances. Rationalists now acknowledge and model the implications of limited cognitions, imperfect information, and heterogeneous motivations. Structure and culture are now explicitly incorporated into many rational choice accounts. Power, institutions, and organizations all matter.

This chapter details significant substantive, methodological, and theoretical advances generated by contemporary scholars using rational choice in comparative and historical analysis. It reveals how rationalists have benefited from debates with (and, sometimes, attacks by) culturalists and structuralists. Ten years ago, the dispute between rationalists and historical institutionalists was raging. There was

some tentative agreement on points of convergence (Levi 1997b; Thelen 1999) but still a wide range of issues on which there was little consensus. Ten years ago, culturalists and rationalists fought over definitions of ethnicity and the role of culture more generally (see, e.g., Ross 1997). Today, there is far more convergence among rationalists, structuralists, and culturalists (Katznelson and Weingast 2005b; also see the chapters by Lichbach, Katznelson and Ross, this volume). Indeed, in writing this chapter, there were any number of authors I had trouble characterizing according to the tropes we used 10 years ago.

While the paradigm wars described by Mark Lichbach (this volume) have certainly subsided, they have not disappeared entirely. Paradigmatic distinctions remain relevant both to training and to research.¹ Younger scholars may be navigating several paradigms at once, but the best of them do so with good training in and understanding of those they use. The chapters in this volume make it clear how the starting point influences the evidence collected and the explanation proffered, no matter the topic. Yet, nearly all the authors find that much of the best work on their subject draws on a multiplicity of methods as well as approaches. It involves, as Alan Zuckerman (this volume) emphasizes, understanding political phenomena as stochastic, multilevel, and involving endogenous relationships. It often involves explanations that rely on the specification of causal mechanisms (McAdam, Tarrow and Tilly, this volume; Zuckerman, this volume; Bates et al. 1998). That said, the comparative advantage of rational choice continues to lie in its commitment to the assumption of individual actors making reasoned choices given the likely choices of others and the contextual and institutional constraints.

The comparative and historical rational choice scholars of the past decade have several features that distinguish their work from that of their predecessors. Their work tends to blur the lines among approaches. It is not uncommon for a scholar to combine rational choice and historical institutionalism, or constructivism, or network analysis, or evolutionary biology. Their work tends to be methodologically pluralistic. Fieldwork, archival research, in-depth interviews, experiments, quantitative analysis, surveys, and formal theory are often combined; not everyone does everything, but everyone seems to do several things. They are also methodologically sophisticated in a way few of their comparativist predecessors were. In addition to the languages and history of specific places and periods, the stuff of comparative politics, researchers learn game theory, experimental methods, or whatever else they need to solve their puzzles. Finally, this generation of scholars is self-conscious about the assumption of rationality itself and sensitive to the cognitive limitations of actors, their ethical commitments, and the processes by which their identities are malleable. There is greater awareness of the role of history and contingency.

¹ The summer institutes on EITM (Empirical Implications of Theoretical Models) call on senior scholars versed in rational choice to train younger scholars, while the recent summer institutes organized by the Max Planck Institute for the Study of Societies draw on historical institutionalists. Several recent edited books further attest to the continuing divide – at least from the perspective of historical institutionalists (Mahoney and Rueschemeyer 2003; Streeck and Thelen, eds. 2005).

A METHODOLOGICAL SHIFT

Since the publication of the first edition of this book, there has been an explosion of work transforming the practice and theory of rational choice approaches in comparative politics. Barbara Geddes's argument (2003) that most work on development is built on sloppy reasoning and biased reading of the evidence, on "paradigms and sand castles," resonates with a generation of younger scholars dissatisfied with development theory or the methodological approaches and theoretical constructs that long dominated comparative politics. Robert Bates (1981, 1991, 1997), David Laitin (1992, 1998, 2002), and Elinor Ostrom (1990) were among those offering an alternative model of how to do comparative research, and their challenge to combine detailed and nuanced empirical research with the analytics of rational choice has now been taken up widely. Simultaneously, Douglass North's emphasis on the importance of time (1981, 1996 [1993]) inspired investigations of institutional change and sequencing using rational choice models combined with an intimate knowledge of history (e.g., Bates 2001; Bates et al. 1998b; Bates, Greif, and Singh 2002; Greif et al. 1994; Kiser 1994, 1997; Kiser and Barzel 1991; Levi 1988, 1997a; Milgrom et al. 1990; North and Weingast 1989).²

Comparativists are drawn to deep immersion in cultures and countries, and they have been inspired – and instructed – by these major scholars using fieldwork and interviews but simultaneously relying on game theory, collective action theory, and institutional analysis. *Analytic Narratives* (Bates et al. 1998b) attempted to lay out the commonalities in the practice of this research program. The aim is to offer guidance on how to combine rigorous modeling with detailed understanding of time and place, the stuff of comparativists and historians.

We now have a cohort of assistant, associate, and even young full professors who are country and regional experts versed in quantitative methods in addition to having strong qualitative and linguistic skills. The rationalists among them have also learned economic theory and game theory and, in some cases, formal modeling techniques. Some do experiments as well. Motivated by a desire to unravel particular puzzles and provide explanations of important phenomena, these scholars have transformed comparative politics. They have also transformed what it means to be a rational choice scholar, at least in comparative and historical research.

How the past influences the present is clearly an issue that has concerned comparativists interested in economic development, state-building, the revival of religious or ethnic hatreds, and a whole host of other central questions. In recent decades, there have been interesting new approaches for discerning how events in the distant past can initiate particular chains of causation that have effects in the present. This is more than the path dependence elaborated initially by Paul David (1985) and W. Brian Arthur (1989) and developed by Paul Pierson

² There is another tradition in comparative politics that also focuses on time, but it is really about conceptions of time rather than change over time. See, e.g., Hanson (1996).

(2004). While there is some debate about the extent to which lock-in and increasing returns play the role assigned to them by these authors (Page 2006), there is little question that interactive processes among individuals and the design of institutions in which they interact raise the costs of changing norms, behaviors, and relationships. The result is a relatively stable set of arrangements. Avner Greif's recent book (2006) illuminates how different institutions and beliefs emerge in different places and times to solve problems of long-distance trade and how stable those institutions and beliefs become in the absence of a major exogenous shock. My own work on military service suggests that differential but systematic historical experiences help explain variation in responses to governmental demands to volunteer or support conscription; how the contract a group made or the treatment the group received at one point in time affects how they react to policy proposals at a much later point in time (Levi 1997a). Both Greif and I engage in historical analysis, but our models and arguments are derived from assumptions about individuals making reasoned and interconnected choices.

Time also plays an active role in cases in which revealed preferences and behavior disguise the lack of change in attitudes and preferences. Timor Kuran's work on unanticipated revolutions that result when individuals learn that others share their views (1989; 1995) is a critical text in revealing how the long-term historical path is not always what it seems to be. A more recent example is Jason Wittenberg's *Crucibles of Political Loyalty* (2006). Wittenberg does not use the terminology of path dependence. Instead, he focuses on the continuity of attachments to parties of the right despite 40 years of Communist rule and relentless efforts to eradicate such attachments. To explain this phenomenon required him to investigate how the church affected the calculations of individuals within the communities under their influence. The church as an institution provided a focal point for interaction, and where its leaders were able to establish a high enough level of religious participation, they were able to insulate the faithful from socialization by and intimidation of the Communist cadres. Wittenberg describes this as a tipping process based on assurances that enough others will participate. Thus, for Wittenberg, ideas and attitudes formed by one generation are transmitted to others over time by means of institutions and networks whose activists are working hard to ensure that they survive. He makes his case with electoral data, archival records, and a complex grasp of Hungarian history, culture, and language. His findings are country specific but also potentially generalizable to pre- and post-Franco Spain and a whole host of other polities that have experienced democratic ruptures.

Wittenberg investigates continuities, but understanding how the past influences the present also requires understanding discontinuities. If Wittenberg is offering us the microfoundations of path dependence, Barry Weingast (2005) is providing the microfoundations of critical junctures. Drawing on his historical research into the interactions between the British and the American colonies, Weingast asks why political entrepreneurs were successful in instigating revolution when they were. He argues that their

effectiveness depended on persuading political pivots that the views of the British held by revolutionary leaders were true. Wittenberg and Weingast do this by pointing to events that confirmed their views, but they can only do this if such events actually took place and if the confirming events sufficiently raised the stakes of the controversy.

There is yet another way to approach these problems of historical effects, one that is also grounded in rational choice but is far more reliant on econometrics than on interpretation of motives or observation of behaviors. For example, the articles by Acemoglu, Johnson, and Robinson (2001, 2002), which appear in economics journals, account for the origins of colonial institutions by considering the geography of settlement locations. A healthy environment produced incentives to settle and, therefore, to self-govern, resulting in a proto-democratic government. An unhealthy environment created incentives to use slave labor and other forms of cheap labor, to exploit natural resources quickly, and in other ways to extract, use up, and leave. The result was a hierarchical economy and polity, often ruled from a distance. The resulting institutions had long-term consequences, affecting the form of postcolonial governments. This set of claims is part of an ongoing debate in economic history and comparative politics concerning the reasons why colonial settlers sometimes imported, sometimes transformed, and sometimes ignored metropolitan institutions and with what consequences for institutional change over time (Engerman and Sokoloff 2008; Engerman et al. 1997; Lipset 1994). There continues to be debate about initial conditions (Przeworski 2004a).

The concept of path dependence originated in economics, was developed most fully by historical institutionalists, and is now being revised yet again by comparative rationalists. The concept of critical junctions emanated from nonrationalist comparative politics (Collier and Collier 1991) but has been transformed by rationalists. Both of these moves have also involved consideration of the role of counterfactuals. Until the publication of Tetlock and Belkin's book (1996), there had been little systematic evaluation of the role counterfactuals play in social scientific analyses. Nor, until recently, has there been much success in giving counterfactual assumptions theoretical justification or bite. In *Analytic Narratives* (Bates et al. 1998b), subgame perfect game theory provides a way to explore the choice among paths, clarify the counterfactual as the path not taken, and illuminate the kinds of evidence needed to explicate the choice. To understand why one of the paths becomes the equilibrium path, it is necessary to understand why the actors did not follow other possible paths. Thus, it is now possible, as Weingast did, to generate testable hypotheses about (1) what the beliefs of the actors had to be and what they could not have been and (2) what the critical junctures were in the decision-making process. It is also possible to challenge this kind of formulation with alternative views of belief formation, as Schieman (2007) does, or by reconceptualizing the parameters, as Greif and Laitin (2004) do. By specifying the model, it is possible to improve the model and clarify what kinds of evidence comparative and historical rationalists require to support their arguments.

Geddes (2003) offers a strong set of claims about what rational choice, and indeed any good empirical analysis, might accomplish and what it cannot. She critiques those who set their sights too high, who try to account for all revolutions or all development; they invariably, in her view, build sand castles subject to the relentless shifts in paradigms. Rather, she argues for more modest, albeit still ambitious, projects. In her own case (Geddes 1994), she identifies the links between electoral institutions and the kinds and timing of reform. If, as Elster (1989b: 7) and Taylor (1988) claim, the best explanations are “fine-grained,” this is a further impetus for defining questions in a way that enables one to spell out the direct relationship between one variable and another.

In general, Geddes has been proved right. The work that best succeeds in meeting social scientific criteria of evidence and falsifiability, the work that can be used as a solid building block rather than a sand castle, is work that precisely models a set of causal mechanisms and clearly delimits their domain of operation. Scholars can still ask big questions but only certain kinds of big questions. For example, Rosenthal (1992) tells us a considerable amount about the relationship between the state, property rights, and economic development by means of a detailed investigation of sewage and drainage in postrevolutionary France. As Migdal (this volume) also notes, studies of seemingly arcane government revenue collection practices contribute to the theory of the state by illuminating the relationships between rulers and constituents and by illuminating sources of political change. Rationalists (Barzel and Kiser 1997, 2002; Bates and Lien 1985; Bergman and Nevarez 2005; Bergman 2003; Cheibub 1998; Gehlbach 2008; Hoffman and Gibson 2005; Kasara 2007; Kiser 1994, 1997; Levi 1988; Timmons 2005; Whiting 2000) have been joined (and sometimes preceded) by a growing number of others who challenge but also sometimes use the rationalist approach (Gould and Baker 2002; Gould 2001; Lieberman 2001b, 2003b; Moore 1998; Steinmo 1993; Tilly 1990). Taken together, this body of work reveals under what conditions governments can more successfully achieve compliance with their tax policies, as well as what kinds of revenue production systems fit best with what kinds of political structures and dynamics. This is also an instance in which scholars starting from different methodological assumptions have learned from each other to the point where it is often difficult to tell who fits in which camp.

The best comparative rational choice work now tends to be multimethod and catholic in its approach to empirical analysis. Rational choice ensures that the research has microfoundations, that is, it pays attention to the constraints on and the strategic interactions among the actors whose aggregated choices produce the outcome of interest. However, the research enterprise itself usually involves some mix of fieldwork, interviews, surveys, archival work, experiments, and statistics in addition to formal logic. The *sine qua non* is an articulated model of rational action with testable implications that are then tested with the most appropriate forms of evidence. If the Green and Shapiro critique (1994) of rational choice’s failure to provide substantiation ever held, it certainly does not hold for contemporary rational choice work in comparative politics.

The demands on scholars are high, but the results are proving the effort worthwhile. Ira Katznelson (this volume) praises the contributions to macro-historical analysis of four major recent works in rational choice: Avner Greif's *Institutions and the Path to the Modern Economy* (2006); Daron Acemoglu and James Robinson's *Economic Origins of Dictatorship and Democracy* (2006); Carles Boix's *Democracy and Redistribution* (2003); and Adam Przeworski, Michael Alvarez, Jose Cheibub, and Fernando Limongi's *Democracy and Development* (2000). These works are the logical result of the long-held conviction by rational choice scholars that their models can be applied to a wide range of problems, places, and groups. Rational choice has the intellectual ambition – some might call it “excessive ambition” (Elster 2000) – to interrogate the most important problems in social science. Success lies in the combination of the rational choice model with the collection and analysis of data.

This is perhaps even more evident in another subject area in which rational choice helps advance the discussion: the sources of civil war and violent conflict. Macrohistorical analysis once seemed the monopoly of structuralists and those engaged in configurational analysis (Katznelson 1997), and studies of conflict dichotomized between those who used the tools of history, historical sociology, cultural theory, and area studies and those whose work abstracted from reality with game and bargaining theory. Several books changed that situation. All investigate particular places and periods, but all have findings that transcend the empirical focus. While contributing to a general theory of conflict and violence, they are also teaching us about the countries and events they study. Roger Petersen's *Resistance and Rebellion* (2001), investigating Eastern Europe; Ignacio Sanchez-Cuenca's *ETA Contra el Estado* (2001), with data on Spanish Basque strategies; Elisabeth Wood's *Insurgent Collective Action and Civil War in El Salvador* (2003); Stathis Kalyvas's *The Logic of Violence in Civil War* (2006), focusing on Greece; and Jeremy Weinstein's *Inside Rebellion* (2007), based on fieldwork in Uganda, Mozambique, and Peru, exemplify how much we can learn about which sides people take and why they kill who they do by investigating their actual interactions, lived experiences, strategies, and organizational resources within a carefully articulated analytic framework. All collect detailed community-level data that allow them to reveal the microdynamics of interactive behavior. Their models are testable and tested, and their data are available for those who want to contest their interpretations. Their work is rooted in rational choice but also reveals how much comparative and historical rationalists have expanded the ambit of rational choice by considering behaviors, such as self-sacrifice, and motivations, such as emotions and identities.³ It is worth considering, as Zuckerman does in the final chapter of the first edition of this volume, the extent to which even their degree of emphasis on instrumental motivations is realistic and appropriate, but it is only because of these research accomplishments that the debate is possible.

Another group of rational choice conflict scholars (Collier et al. 2003; Collier and Sambanis 2002; Elbadawi and Sambanis 2002; Fearon 2004; Fearon and

³ Ross (this volume) cites Petersen's second book (2002) for his narrative of emotions.

Laitin 1996, 2000b, 2003, 2004; Hegre and Sambanis 2006; Sambanis 2001, forthcoming), using large-N databases, albeit often supplemented by detailed case studies, address variations in violence across countries and time. They, too, formulate arguments about the origins and trajectories of conflicts, but they are able to engage in large-scale comparisons of a sort not possible by those engaged in a more ethnographic endeavor. Despite controversy over the quality of the databases and the findings in the analyses, there has been considerable progress in developing better tools for analyzing conflict. Rational choice comparativists have played an important role here, but they – as in many of the areas researched by comparative rationalists – are engaged in an ongoing conversation and often collaborations with those using other approaches.⁴

Since the publication of the first edition of this volume, there has been real progress on a wide range of problems by scholars grounded in rational choice (and, often, other paradigms as well) but also producing original data and empirical research. Issues on which advance has been made include institutional change and variation (Bates et al. 1998b; Farrell 2005; Jupille 2004; Luong 2002; Ostrom 2005; Schofield 2006); legislatures, parties, and elections (Carey 1998, 2000; Cheibub 2007; Cox 1997; Huber and Shipan 2002; Magaloni 2006; Smyth 2006; Tsebelis 2002; Tsebelis and Money 1997; Tucker 2006; also, see Anderson and Huckfeldt this volume); the role of labor unions and business groups in government policy (Garrett 1998; Golden 1997; Iversen 1999, 2005; Mares 2003, 2006; Murillo 2001; Wallerstein and Western 2000; also, see Mares this volume); processes of development and democratization (Cleary and Stokes 2006; Geddes 1999; Gibson 1999; Gibson et al. 2005; Wantchekon and Neeman 2002; and see Solingen this volume for discussion of the developmental state); political and economic reform (Bates 1998, 2001; Shleifer and Treisman 2000; Stokes 2001a); federalism and decentralization (Díaz Cayeros 2006; Rodden 2006; Rodden and Wibbels 2002; Treisman 2007); and ethnicity and national identity (Chandra 2004; Hechter 2000; Laitin 1998, 1999; Posner 2004, 2005; Wilkinson 2004).

Several factors have contributed to the advances in comparative rational choice. First and foremost is the increased methodological sophistication and pluralism. Also important is the extent to which many rationalists have taken on board the challenges posed by historical institutionalists, interpretivists, and bounded rationalists. Much of the work cited previously builds on non-rationalist as well as rationalist comparativists. For example, Kanchan Chandra (2004) relies on constructivist progenitors in modeling ethnicity, Jonathan Rodden (2006a) on classical political theory in thinking about federalism, and Susan Stokes (2001a, 2001b) on the public opinion research tradition to explain citizens' responses to neoliberal reforms. Camps have learned from each other, and the result is an increase in the knowledge of given problems and subjects while improving our capacity as social scientists to understand institutional development and change.

⁴ See, e.g., Michael Ross (2006) and Jack Goldstone (2001), both contributors to this research program, for reviews of the recent literature. Also see Zuckerman's chapter in this volume.

Yet, as Lichbach (this volume) points out, there is still a divide. Not all comparative rationalists rely on mathematical formulations or formal game theory, but they do use the logic of collective action, spatial models, bargaining, and strategic interaction in a sophisticated and informing manner. Those using other approaches generally eschew these tools, but important exceptions (e.g., Carpenter and Lewis 2004; Thelen and Kume 1999) suggest that even that claim stands on shifting ground.

The new comparative and historical rationalists are also the beneficiaries of the transformation of area studies (Hanson forthcoming). It is no longer enough for a comparativist to be an expert on a place; she or he must also use that knowledge to ask larger social science questions. A comparative rationalist who is expert on a given country or region must, in addition to acquiring linguistic and fieldwork skills, understand transaction costs, bargaining models, and collective action theory. Even among those who use game theory to establish their analytic frame there is a reliance on area knowledge, historical research, and qualitative methods. Comparative and historical rationalists are filling in gaps in the continuum from abstract mathematical formulations to in-depth contextual analyses.

It is hardly surprising that so much of the recent work is book length. The kinds of evidence demand considerable data collection. The arguments require nuanced elaboration. They need to situate the interactions observed in the history, politics, economics, and societies of the particular countries and periods from which the data are being collected.

In the rest of this chapter, I consider the intellectual sources and methodological innovations that make such ambitious research possible. I then take up the limits of rational choice and which of those might yet be overcome.

THE MODEL OF RATIONAL CHOICE: NEITHER NEOCLASSICAL ECONOMICS NOR PUBLIC CHOICE

Influences

Most of the foundational work in rational choice derives from the neoclassical economic model in which the outcome of analytical interest is the consequence of the aggregation of the decisions of many rational individuals maximizing their egoistic interests. Rational choice theory is not, however, neoclassical economics (Bates 1988). Its attention to institutions and norms, to the richness of context, to questions of conflict and power, and, on occasion, to nonegoistic motivations differentiate it sharply from the straightforward application of economics to politics that characterized the early public choice school. Moreover, its aims are empirical and explanatory rather than normative and exhortatory. Whereas James Buchanan and Gordon Tullock use their models to attack the role of the state in the economy, the practitioners of rational choice seek to explore and understand what role the state, politics, groups, and organizations have actually played in economic and political development.

There are four distinct sources of contemporary comparative rational choice. Much of this work has influenced economics as well as political science, but my interest here is only in the scholars and arguments that have produced contemporary comparative and historical rational choice. The first source is the spatial analyses pioneered by Duncan Black (1958) and Anthony Downs (1957) to account for electoral outcomes. In the hands of William Riker (1962a, 1964, 1982) and those directly influenced by him, spatial modeling also became an invaluable tool in the investigation of the origins and maintenance of such political institutions as federalism and cabinet coalitions (e.g., Laver and Shepsle 1994; Schofield 1987; Schofield and Sened 2006; Weingast et al. 1995). The second source is the social choice framework provided by Kenneth Arrow (1951). When individuals operate within the framework of the free market, the result is both personally and socially efficient – or so the neoclassical claim goes. In contrast, Arrow illuminates how the institutions of politics do not necessarily lead to Pareto-optimal outcomes; the combined decisions of rational individuals often generate nonrational collective choices. While Arrow's own work on democratic institutions is more normative than empirical, it has nonetheless motivated important research on cycling within legislatures. Arguably, the most important scholar building on Arrow's insights is Norman Schofield (2006), whose recent book significantly advances the agendas of both Riker and Arrow in its investigations of political change.

The third major inspiration is collective action theory, popularized by Mancur Olson (1965) and elaborated by Michael Taylor (1987 [1976]), Russell Hardin (1982), Robert Axelrod (1984), and Mark Lichbach (1997b), among others. The emphasis is on strategic interaction; each individual makes her or his decision based on the probable decision of others. Olson's conception of the free rider problem and its effects reduces to a fiction models based on the automatic translation of interests into organization. Simultaneously, it stimulates systematic investigation of the conditions under which individuals will join together to influence government or each other and the conditions under which it is even possible for government and other organizations to deliver what constituents demand.

The fourth source for contemporary comparative rational choice is the new economic institutionalism, credited to Douglass North (1981, 1990). This changed how scholars theorized the constraints on action and the costs of certain choices. North's promotion of transaction cost theory was combined with the recognition of the role of relative bargaining power. This approach stimulates systematic investigations of the variation in both state action and outcomes throughout history.

Olson and North share with Arrow the recognition of inefficient outcomes, but their goal is more empirical: to understand when such results occur and why they do not always. For Olson, the reason that optimal or more proximately optimal outcomes are sometimes achieved rests in the provision of selective incentives. For North, the reason that we can explain structure and change rests in institutions and norms that constrain or facilitate choices, on

the one hand, and that increase or decrease the transaction costs, on the other. Both North and Olson give politics an important role in economic analysis; actors with distributional agendas can influence the choices rational individuals make by altering the relative prices of the options in their choice set. For example, union officials can make it more rational for potential members to join and pay dues by controlling access to employment (Olson 1965), or rulers can create secure property rights that influence where investors place their funds (North 1981).

Comparative and Historical Rational Choice

Empirical comparative and historical rational choice is based on the assumption that individuals are rational in the sense that they choose within constraints to obtain their desired ends and strategic in that these decisions rest on their assessment of the probable action of others. The outcome depends on the aggregation of these individual actions. This means that the individual intentions of actors often lead to unintended or undesired consequences (a feature misunderstood by Migdal this volume). Rational choice is concerned fundamentally with bargains, strategic interactions, transactions, and institutions and, increasingly, with incorporating research on cognitive capacities and limitations, information, and networks. The approach is methodologically individualist, yet its focus is on the aggregation of individual choices rather than what goes on in the mind of any particular person. Rational choice is, as Lichbach (this volume) notes, a model that offers the microfoundations of macroprocesses and events. Key to the model are the assumption of rationality, the nature of the constraints on behavior, the determinants of strategic interaction, and the explanation of why and when equilibria are possible and stable and when they are not.

There are real questions about what it means to be rational and of how much it matters whether individuals are actually rational or not. Most rationalists continue to rely on a fairly thin variant in which individuals act consistently in relation to their preferences. The trick is in defining the preferences in general, *ex ante* to a particular application. In many instances, this is not difficult, requiring only a fairly straightforward observation of what is of principal concern to the class of actors under consideration. For example, if they are economic entrepreneurs, maximizing wealth seems a reasonable assumption. If they are peasants in risky environments, maximizing security has some appeal. In the case of government actors, the presumption is that they want to stay in power. The utility functions of citizens are trickier to clarify given the range of interests they might have; there is nothing comparable to the economics dictum of getting the most for the least money in the marketplace. Thus, any maximand will lead to hypotheses that can account for a smaller part of the variation in citizens' behavior than in politicians' or economic consumers' behavior.

Rational choice is not bound by the utility or wealth-maximizing assumptions that characterize economics. Rational choice does not even require the

assumption that individuals are self-interested. Both Amartya Sen (1977) and Russell Hardin (1988) rely on models of rationality in which others are taken into account. Revision of the standard economic maximand is common in rational choice generally but is a particularly salient feature of much of the new comparativist and historical work. My account of contingent consent to military service assumes that actors also have ethical commitments (Levi 1997a). The Cook, Hardin, and Levi approach to trust (2005) presumes that trustworthy individuals have motivations that include the well-being of the truster. As noted earlier, a lot of the work on ethnicity or conflict presumes that individuals have motivations rooted in identity, fear, or revenge. There are trade-offs here, however. While the assumption of utility maximization is both parsimonious and extremely general, it produces tautology: Whatever people do becomes a “revealed preference.” The advantage of a more specific wealth- or power-maximizing assumption is that it produces observable implications but at the cost of a narrow conception of human motivation. The addition of nonegoistic considerations, motivational norms, or emotions increases the complexity and difficulty of the analysis. The advantage of an assumption in which actors consider net payoffs that include both material and ethical factors is that it may better approximate reality, albeit at the sacrifice of a neater and more parsimonious model.

The real action in the model does not, however, come from the internal considerations of the actor but from the constraints on her or his behavior. Constraints have two major sources. The first, derived straightforwardly from neoclassical economics, is scarcity; individuals maximize within the confines of their available resources. An individual without money cannot make the investments that would make her richer, and an individual who lacks the vote cannot directly influence who is in government, no matter how much it might be in his interest to elect a certain party. Another set of important constraints are institutional and organizational. What precisely defines an institution remains an ongoing subject of reconsideration. Whether they are rules (North 1990; Ostrom 2005) or a set of factors that motivate regularity of behavior (Greif 2006), they structure social interactions and produce equilibrium outcomes, that is, outcomes that no one has an incentive to alter.

The third component of the approach is its investigation of the conditions that produce an equilibrium, a position reached by individuals interacting until they can find no position that would be better. Sometimes, of course, there is no such position or it is impossible to achieve; then the result is either cycling or chaos. Sometimes, there is a range of possible positions, and how a particular coordination point is chosen becomes part of the object of study. Equilibrium analysis does not imply that all behavior is static, nor does it suggest that all interactions among rational individuals produce an equilibrium. Arrow’s paradox and other important findings in rational choice rest on the opposite finding.

Nor are all equilibria efficient. The research on collective action problems is an explicit recognition of the conditions under which the logic of rational action

is likely to produce suboptimal equilibria and of the kinds of arrangements that make it more likely for rational individuals to produce more optimal equilibria. Ostrom (1990) points to the herders of Torbel, Switzerland, and the *zanjera* irrigation systems of the Philippines as examples of long-enduring common pool resource (CPR) problems, but there are also groups – she points to cases in Sri Lanka, Turkey, and California – who have trouble finding and sustaining solutions to their CPR problems. In my work (1997a), volunteering proved an inefficient equilibrium, at least from the point of view of the military, during a large-scale war, but altering that equilibrium took substantial political effort by legislators and other government officials who shared the military's view.

Rationalists have long recognized the importance of understanding equilibrium change, but their analyses have generally involved comparative statics rather than a more dynamic approach (see the discussion in Bates et al. 1998b: 232–233). One of the many reasons for the wide interest in the recent books by Greif (2006) and by Acemoglu and Robinson (2006) is that they theorize endogenous sources of change. They attempt to show how previous institutional arrangements lead to shifts in resources and equality that then promote demands for institutional change.

Strategic interaction is the final pillar of the rational choice approach. In the powerful price theory of neoclassical economics, the consumer lets the market transform the choices of others into a price and a range of products that form the basis of her or his choice. The actor does not have to consider what others are going to do before choosing items on the shelf. In most political and social situations, however, an evaluation of the behavior of other relevant actors comes prior to the choice. The decision whether to free ride, overfish, or pasture too many cows depends, at least in part, on an assessment of what others are likely to do. The vote of a legislator, the compliance of a citizen with government, the willingness to engage in collective action or even rebellion are all acts that generally depend on beliefs about the likely actions of others. A young man may want to volunteer for military service but only if he believes others will join him on the front lines. An assessment that too many potential soldiers will free ride may transform an opponent of conscription into a supporter.

The outcome for all depends on the choice of each. In order to make a choice in a context of strategic interaction, each decision maker has to have expectations about the other players. Traditionally, rationalists assumed that individuals presumed that others were also instrumentally rational and would thus draw the same inferences from the same information. More recent research is concerned about what kinds of information and expectations actors have and what they think others know, expect, and believe. Indeed, some of the most cutting-edge research raises these questions. Rationalist attempts to understand why populations waited so long to protest against disliked regimes (Kuran 1995) rely on informational cascades and other tipping processes (also see Baldez 2002). Experiments are now supplementing fieldwork in order to establish how beliefs about others influence cooperative behavior among strangers in Kampala, Uganda (Habyarimana et al. 2007).

THE LIMITS AND POSSIBILITIES OF COMPARATIVE RATIONAL CHOICE

Comparative and historical researchers, who wish to combine a nuanced understanding of the complexity of a particular (often unique) situation or set of events with a general theoretical understanding, demand more of rational choice than it has so far delivered. First, neoclassical economics and its increasing mathematization and formalization is powerful and parsimonious, but it tends to rely on stylized facts rather than on the observations and details that enrich both the narrative and our confidence that we have explained an actual occurrence. Second, even when a rational choice analysis offers a logical story consistent with the facts, this hardly constitutes a validation of the explanation. Third, the presumption that changes in behavior reflect changes in relative prices, that is, the emphasis on tangible incentives, fails to capture some of the most interesting behavior, such as large-scale collective actions or cooperation within firms or with government. Fourth is the concept of power, which is central to political science. Yet, there remains the question of how best to deal with power in a rationalist framework.

The first two problems are clearly the most tractable, and, as this chapter shows, there has been considerable progress since the last edition. The others are more difficult.

Reconsidering the Behavioral Assumption

Herbert Simon won the Nobel Prize for recognizing the demands rationality places on the calculative capacity of the decision maker. Strategic action increases the requirements, for individuals must figure out what others are likely to do in order to devise their own best strategy. The assumption of common knowledge among actors about each other includes as given a considerable amount of information and fails to reckon the costs of acquiring such information. For many of the actors and in many of the situations of interest to comparativists, institutions do much of the work of calculation and information provision. They have this effect because they convey expectations both about sanctions and about the behavior of others. This information then enables each actor to make an informed strategy choice. For example, the institution of the income tax informs potential taxpayers of the nature and probability of punishments for personal evasion, but it also lets them know that most others cannot or are unlikely to free ride. The information has a deterrence role and, at the same time, may encourage reciprocity as a motivation for payment (Levi 1988).

Institutions solve some of the informational problems actors face but hardly all. Recent work on networks offers some additional insight into both how individuals interact strategically and how they obtain and use information. Both Robert Huckfeldt and Alan Zuckerman address this literature in their contributions to this volume, and both make the important point that channels of credible communication depend on patterns of social relationships and learning

that comparative rationalists are only just beginning to recognize (also see Zuckerman 2008; Zuckerman, Dasović, and Fitzgerald, 2007).

The narrow behavioral assumption of most rational choice analysis is more difficult to overcome. Behavioral economics, experimental research, and new advances in the program of bounded rationality inaugurated by Herbert Simon (see Zuckerman this volume for a review of this literature) are raising serious issues for rational choice scholars. Concerns with fairness, the role of emotions, and cognitive limitations affect choices and behavior in ways that some rational choice scholars are now attempting to incorporate in their models and analyze empirically.

The challenges to the underlying assumptions of rational choice from cognitive psychology and behavioral economics have caused some eminent rational choice scholars to revise their thinking significantly (North 2005; Ostrom 1998) and even, in some cases, to recant (Taylor 2006). For North and Ostrom, it is possible to use a revised behavioral assumption and an enriched understanding of cognitive limits to build a more sophisticated model of choice. In this they are joined by scholars, such as Bryan Jones (1999, 2001), who have long relied on Simon in their approach to empirical analyses of decision making. For Taylor, the critique is devastating on both logical and empirical grounds; he sees no way to bridge the divide between what we now know about human motivation and cognitive limits and a rational choice approach as he understands it.

Taylor's position is extreme and fails to acknowledge the advances by scholars who have begun to explore the role of culture, norms, mental models, and other ideational and cognitive factors in influencing institutional and regime change.⁵ Christopher Anderson (this volume) outlines the importance of this move in rationalist accounts of mass behavior and voting. John Ferejohn's (1991) study of Parliament in early Stuart England supplements the analysis generated by rational choice with an analysis generated by an interpretive approach as a means to offer a more complete explanation. This perspective has clearly influenced comparative rationalists, as evidenced in much of the work I have discussed. Interpretation now supplements game theory in understanding the strategic manipulation of beliefs among Serbians under Milosevic (Bates et al. 1998a), by potential American revolutionaries (Weingast 2005), among the members of unions in the United States and Australia (Ahlquist and Levi 2008), in considerations of constitutional change (Schofield 2006), and in accounting for the success of Maghribi traders in the medieval Mediterranean (Greif 2006). Other comparativist rationalists are exploring the sources and role of beliefs in a wide variety of settings using fieldwork techniques (e.g., Ensminger and Knight 1997; Horne 2006; Levi and Sacks 2006; Manion 2004). Yet another path involves following the lead of behavioral economists and using experimental research in a wide range of contexts to sort out when considerations of equity, reciprocity, and fairness

⁵ But see Ross (this volume) and Lichbach and Seligman (2000) for the tensions between the individualist approach of rationalists and the more collectivist approach of culturalists.

affect political choices and behavior (Ensminger 2004; Habyarimana et al. 2007; Henrich et al. 2004; Ostrom and Walker 2002). This is a promising enterprise that is just getting underway in comparative politics.

Power

Terry Moe (2005) compellingly critiques rational choice accounts for giving short shrift to power. While there is some attention to bargaining power, most rational choice accounts emphasize the goal as being a cooperative solution in which all involved are better off. There is far too little attention to how the bargain is imposed, who is left out, and who is made worse off. His definition of power revolves around “. . . agenda control in which one actor denies the status quo to others in order to steer them into accepting alternatives more to his liking” (Moe 2005: 227). He notes a few partial exceptions (and I am happy to find my own work among them). There have certainly been attempts at rectifying this lacuna, most notably in the work of theorist Jack Knight (1992), in efforts to absorb the findings of exchange theory into political science (Cook et al. 2005), and in arguments by Henry Farrell concerning interfirm relationships in industrial districts in Europe (2004, forthcoming). However, Moe chides even us for failing to adequately absorb power into the analytics of rational choice.

His point is well taken. In reviewing the wide range of books and articles on inequality, regime change, development, and many other topics, one finds discussion of allocation of resources, bargaining strategies, dominance, and coercion. Yet, power remains undertheorized. For example, Greif mentions the importance of hierarchical power (Greif 2006: 34–35), but it is one of the few concepts he fails to elaborate. For Acemoglu and Robinson, political power plays a critical role in accounting for policy and institutional change, but in the end, their definition is a technical rather than a substantive one: “. . . political power is anything that enables a social group to come close to its preferred policies, *de jure* or *de facto*” (Acemoglu and Robinson 2006: 174). We may be able to excuse Greif, Acemoglu, and Robinson; after all, they are economists by training. Rationalist political scientists do no better, however. This is partially a result of the parsimony of game theoretic models in which only the primary actors of interest are modeled. It is partially a result of focusing on outcomes rather than asking questions about losers and winners.

Comparative rationalists are flirting with power, but they are not addressing it head on or making it a centerpiece of explanation or study. Yet, power is key to understanding both behavior and institutions, and changes in power relations are a major impetus for change. Who has power and how they use it (and cannot use it) tells us a lot about the nature of the society in which people live and the choices they can (and cannot) make. Moreover, theorizing power is one of political scientists' comparative advantages relative to the economists. Adopting rational choice, even in its most modern and nuanced variants, without also theorizing power will give us but partial answers to the puzzles that plague us.

CONCLUSION

Empirical rational choice in comparative analysis was in its relative infancy when I wrote for the first edition of this volume; now it is well into childhood, if not young adulthood. It has become one of the leading paradigms in the field and has produced major and influential work on a range of subjects, places, and periods. Although the divide between rationalists and many of those in this volume may be shrinking as rationalists become more concerned with context and nonrationalists recognize collective action problems, voting cycles, and other insights from rational choice, the divide remains nonetheless. Structuralists and rationalists are in the same conversation, but they nonetheless persist in holding very different views of the origins and effects of institutions and preferences. Many rationalists are taking on board the concerns of culturalists and interpretavists with providing a more complete account of preferences and strategies, but they continue to disagree on the uses of those accounts. What divides rationalists from culturalists and structuralists is not method in the sense of mathematics versus statistics, fieldwork, observation, and archival research; there are many rationalists who rely on precisely these tools. What divides them is method in the sense of how to construct theory and organize research findings. Rationalists continue to emphasize methodological individualism and strategic interaction.

Rational choice will continue to have its serious detractors in comparative politics. It simplifies the world and human psychology more than suits the tastes of many comparativists, especially those committed to area studies or interpretive explanations. Its positivist ethic may be unpalatable to postmodernists and others. The very commitment of rationalists to scientific progress by means of fact-finding, testability, and partial universalism will remain repugnant to some critics and an impossible goal to others. Rationalists must continue to refine and clarify their models so as to increase their explanatory power, and they must find more satisfying means for arbitrating among competing accounts. These are the tasks incumbent on all comparative social scientists committed to explanation. Continued competition among paradigms may improve our ability to accomplish such tasks.

The argument of this chapter has been more modest, however. The commitments of this generation of comparative rationalists to understanding context and to collecting original data have already contributed to a much richer approach and one that is beginning to deliver on the promise of even better rationalist theory – and far better problem solving – than their predecessors. With the absorption of advances in cognitive theory and network analysis, and with the development and use of more nuanced theories of power, the next decade should prove even more exciting.

Culture in Comparative Political Analysis

Marc Howard Ross

INTRODUCTION

This chapter argues that culture is important to the study of politics because it provides a framework for organizing people's daily worlds, locating the self and others in them, making sense of the actions and interpreting the motives of others, for grounding an analysis of interests, for linking identities to political action, and for predisposing people and groups toward some actions and away from others. Culture does these things by organizing meanings and meaning-making, defining social and political identity, structuring collective actions, and imposing a normative order on politics and social life.

To examine how culture operates, we must approach it through the formal and informal verbal and nonverbal narratives about the social and political worlds that people who are part of a culture share. However, before I discuss culture as a useful, and underused, perspective in comparative politics, three caveats are in order. First, to be useful, culture cannot be defined so broadly as to include all behaviors, values, and institutions lest it lose any distinctiveness and explanatory power. Second, cultures are not formal units with delimited clear borders and membership cards; nor are they fully integrated internally or always internally consistent. Rather, their boundaries, integration, and consistency are regularly subject to contestation. Third, the effects of culture on collective action and political life are generally indirect, and to fully appreciate the role of culture in political life, it is necessary to inquire how culture interacts with, shapes, and is shaped by interests and institutions.¹

It should not be surprising that cultural analyses in comparative politics take many forms, for unlike rational choice theory or institutionalism, cultural approaches are less clear about the domains of politics that are most important to study and the kinds of explanations to offer. There is even less consensus concerning the methods and tools to employ. Lichbach (this volume) distinguishes between subjective and intersubjective views of culture: The subjective

¹ Berman (2001: 241–244) appropriately notes that we should not assume that culture necessarily affects political life, but rather that we need to examine if, when, and how it does.

emphasizes how individuals internalize individual values and attitudes that become the object of study, while the intersubjective focuses on the shared meanings and identities that constitute the symbolic, expressive, and interpretive part of social life (Green 2002a: 9; Klotz and Lynch 2007: 7–9). I argue here for the merits of a postmodern intersubjective understanding of culture (with attention to subjective elements) and against the value of viewing culture as one or more distinctive traits or values that are decomposable into individual-level measurable traits or values. This strong view of culture is completely compatible with the belief that comparison is central to the social science enterprise while not denying its complexities. In making this argument, an important task is to situate recent works in the field in terms of crucial questions that cultural approaches to politics address. However, this chapter is not a review, and there are many additional studies that would be included if reviewing the field were my goal. Rather, it offers an opportunity for me to articulate and elaborate on what an intersubjective cultural approach can contribute to comparative politics. At the outset, it is worth noting that in 1997 I argued that cultural contributions to comparative politics were relatively rare and far less developed than rational choice or institutional approaches. This is not as true today, as scholars in the field and graduate students now take culture more seriously than was the case earlier, although I remain concerned that this is too often done superficially.

Rather than repeat many of the points from the earlier chapter (Ross 1997), I emphasize developments in the field since then and develop more fully parts of the earlier argument that I either had not touched upon or treated briefly, including the central role of narratives in cultural analyses; the power of cultural expressions and enactments in identity-based conflicts; and points of convergence and differences between cultural, rational choice, and institutional frameworks.

Culture is not a concept with which most political scientists are comfortable (Norton 2004). For many, culture complicates issues of evidence, transforming hopes of rigorous analysis into “just-so” accounts that fail to meet widely held notions of scientific explanation (King, Keohane, and Verba 1994: 109–110). Culture violates canons of methodological individualism while raising serious unit-of-analysis problems for which there are no easy answers. To many, neo-Marxists and non-Marxists alike, culture seems like an epiphenomenon offering a discourse for political mobilization and demand-making while masking more serious differences dividing groups and individuals. In addition, cultural analyses raise questions about the mechanisms by which culture shapes politics and challenge widespread assumptions about the causal importance of interests and structures. Each of these potential issues is addressed in this chapter, and while I am not arguing that they are unimportant, I do not view them as sufficiently problematic to warrant throwing the baby out with the bath water. At the same time, I want to be clear at the outset that my argument that culture can illuminate micro- and macro-level political dynamics and provide explanatory mechanisms not revealed by either rationality or institutionalism does not mean that culture always matters or that it

necessarily matters more than other factors. If, and how, culture matters is often a function of context and structure (Posner 2004).² Saying that culture matters is not to say that cultural differences matter; rather, it is to recognize that people sometimes mobilize around differences large and small, but this does not mean they are the source of conflict. For one thing, within-culture conflicts are often the most bitter of all, and there are innumerable cases in which culturally different groups live together peacefully for long periods (Horowitz 1985: 113–124). Instead of thinking that cultures and cultural differences cause conflict (Eller 1999; Posner 2004), we should see cultures as the lenses through which the causes of conflict and mobilization are refracted (Avruch and Black 1993: 133–134).

Cultural analyses of politics challenge the field's preference for methodological individualism and take seriously the postmodern critique of behavioral political analyses, offering the potential for contextually rich intersubjective accounts of politics that emphasize how people understand social and political action (Merelman 1991). In cultural analyses, for example, interests are contextually and intersubjectively defined and the strategies used to pursue them are viewed as context dependent. Narratives – stories, if one prefers – that individuals and groups recount to make sense of their social and political worlds are grounded in the interpretations held by people and groups, their motivations, and events they experienced, and are central to cultural analysis.³

Here I use shared narratives and interpretations – worldviews – mainly to refer to the intersubjective meanings of actors, but they also are ways that social science observers communicate their understanding of events to others (Green 2002: 6; Klotz and Lynch 2007: 12–13; Taylor 1985). Narratives and interpretations both serve as mechanisms linking thought and action and are important methodological tools.⁴ In addition, culturalists pay attention to ritual and symbolic cultural expressions and enactments in everyday life, special occasions and extraordinary moments in studying connections between systems of meaning, the structure and intensity of political identity, and political action.

This chapter has four parts. The first discusses the concept of culture as shared meaning and meaning-making and identifies five contributions analyses of culture have made to comparative political analysis: (1) framing the context in which politics occurs; (2) linking individual and collective identities; (3) defining

² Harrison and Huntington (2000) edited a collection of essays called *Culture Matters*, in which a number of the authors employ a weak trait-based definition of culture and then some provocatively assert that “culture makes almost all the difference.” Many of the analyses emphasize bivariate correlations, often at the country level, and communicate too little about the mechanisms underlying cultural effects or the within-country variation on the variables analyzed, such as specific values and wealth. What I aim to do here is to avoid this sort of “my variable matters more than your variable” claim, and to suggest that it is more productive to think about *when* and *how* culture matters and how culture interacts with structural and strategic variables to help us answer important questions in comparative politics.

³ Sometimes interpretations are best viewed as building blocks for narratives; however, at other times, deeply held narratives give rise to interpretations.

⁴ In this chapter, I refer to both narratives and scripts. In my usage, narratives are the larger accounts and scripts are more specific action-oriented elements found in them.

boundaries between groups and organizing actions within and between them; (4) offering a framework for interpreting the actions and motives of others; and (5) providing resources for political organization and motivation. The second part explains the importance of narratives and interpretations as a way to link the contextually rich details found in particular political settings (be they small communities or countries) to the comparative study of collective behavior such as ethnic and national conflict. The third part considers five critiques of cultural studies of politics: (1) unit-of-analysis issues; (2) the problem of within-culture variation; (3) the difficulty of distinguishing culture from social or political organization; (4) the static nature of culture and its inability to explain political change; and (5) the need to identify underlying mechanisms that suggest “how culture works.” Fourth, the conclusion explores some compatibilities and incompatibilities between cultural analyses and rational choice and institutionalist approaches and concludes that culture is too often ignored as a domain of political life that can enrich how we conceptualize areas such as political economy, social movements, and political institutions in a number of useful ways, often complementing the insights derived from other approaches.

CULTURE AND CULTURAL ANALYSES OF POLITICS

Culture, a central concept in anthropology, has been defined in a wide variety of ways that variously emphasize culture as social organization, core values, specific beliefs, social action, or a way of life (Kroeber and Kluckholm 1952). Most contemporary analyses, however, begin, as I do here, with Geertz’s definition of culture as “an historically transmitted pattern of meaning embodied in symbols, a system of inherited conceptions expressed in symbolic forms by means of which men communicate, perpetuate, and develop their knowledge about and attitudes towards life” (1973c: 89).⁵ This view emphasizes culture as “webs of significance” that are public, shared meanings, not a collection of discrete traits whose integration is presumed; behaviors, institutions, and social structure are understood not as culture itself, but as culturally constituted phenomena (Spiro 1984). Aronoff writes that “Traditional approaches define political culture in terms of attitudes and values, whereas more contemporary approaches view culture in terms of scenarios and discourses” (2001: 11643).⁶

Culture, from this perspective, is a worldview containing specific scripts that shape why and how individuals and groups behave as they do, and includes both cognitive and affective beliefs about social reality and assumptions about when, where, and how people in one’s culture and those in other cultures are likely to act in particular ways (Berger 1995; Chabal and Daloz 2006; Schweder and

⁵ D’Andrade (1984: 88) points out the radical shift from the view of culture as behavior that could be understood within a stimulus-response framework to culture as systems of meaning, as found in a number of fields. For a more complete discussion of culture as meanings and symbols, see the excellent discussions in Schweder and LeVine (1984).

⁶ For additional critique of the trait based approach see the chapters by Barth (1999) and Urban (1999) in Bowen and Petersen (1999).

LeVine 1984). For purposes of political analysis, I want to emphasize that shared understandings are found among people who have a common (and almost invariably named) identity that distinguishes them from outsiders. Culture, in short, marks what people experience as a distinctive way of life characterized in the subjective we-feelings of cultural group members (and outsiders) and expressed through specific behaviors (customs and rituals) – both sacred and profane – that mark the daily, yearly, and life cycle rhythms of its members (Berger 1995). Cultural symbols, metaphors, and narratives have cognitive meanings that describe group experiences, high affective salience that emphasizes unique intragroup bonds that set one group's experience apart from that of others, and scripts that direct action. People draw upon them to explain the past, interpret the present, and evaluate future action.

It is important to note that even when different individuals understand each other and share a common identity, this does not signify that widely held meanings are necessarily acceptable to all, that all who define their shared identity behave in the same way, or that all hold it equally intensely. Rather, there are often intense intracultural differences and conflict over these matters so that meaning and identity, control over symbols and rituals, and the ability to impose one interpretation rather than another on a situation are frequently bitterly contested (Norton 2004; Ross 2007; Scott 1985). In this same vein, Laitin contends that culture highlights points of concern to be debated (1988: 589) and not just areas of agreement. Participating in the same culture or sharing a cultural identity does not mean that people need to be, or necessarily are, in agreement on specifics, only that they possess a similar understanding of how the world works (Aronoff 2001: 11640).

Some scholars, such as Wedeen (2002), argue that Geertz overemphasizes the integration of culture and fails to consider ways that it is both contested and never fully bounded (also see Fearon and Laitin 2000; Norton 2004). She contends, therefore, that culture should be approached as semiotic practices of meaning-making that emphasize what language and symbols do but also the effects of institutional arrangements, structures of domination, and strategic interests (Wedeen 2002: 714; also see Chabal and Daloz 2006). It “designates a way of looking at the world that requires an account of how symbols operate in practice, why meanings generate action, and why actions produce meanings when they do” (2002: 720). Wedeen argues that emphasizing the meaning-making practices helps to better understand the politics and consequences for how particular meanings become authoritative and identity construction.⁷ Others, such as Aronoff (2001), Bourdieu (1977), Foucault (1979), Norton (2004), and Swidler (1986), also emphasize culture as practice, referring to both

⁷ While there is much that is useful in Wedeen's emphasis on a semiotic approach to culture, as well as her critique of most political culture research and Huntington's (1993) clash-of-civilizations argument, I find her critique of Geertz too strong in places, particularly in faulting his influence for the weakness in earlier political science uses of culture – especially those that appeared more than a decade prior to Geertz's (1973a, 1973c) most influential work on culture.

meaning-making and political relationships that privilege certain actions and groups over others. Ortner (1997) adds that not only are cultures not fully integrated and bounded groups despite the fact that common language usage essentializes culture, but furthermore, in the contemporary globalized world, people sharing a cultural identity and its practices can be highly dispersed and diverse in ways that earlier trait-based or physically bounded conceptions of culture did not consider.

Placing the concept of culture at the center of analysis affects the questions asked about political life (Brysk 1995; Merelman 1991; Wedeen 2002). For example, an interest in distinctive worldviews and identity leads to questions about how differences in worldviews might explain such phenomena as the emergence of particular leaders, their exercise of power and authority, the organization of political decision making, social movement mobilization, or the perception of external threats. At the same time, an interest in culture discourages inquiry into the role of rational self-interest in political choice-making when such questions presume that interest maximization is more or less invariant across cultures and does not need a theory of cultural variation to explain what is viewed as constant (Wildavsky 1987).

Culture contributes to the study of comparative politics in at least five distinct ways. I present these more briefly than in the earlier chapter (Ross 1997), identifying each briefly to help a reader to understand the approach to culture and politics I develop later and how it differs from most studies associated with the term “political culture.” This work is often associated with Almond and Verba’s *The Civic Culture* (1963) and many later studies in the same mold (e.g., Inglehart 1988; Verba, Nie, and Kim 1978). Utilizing data collected from large national samples, the authors sought to explain democratic effectiveness and participation as a function of differences in political culture defined as the particular distribution of patterns of orientation toward political objects that citizens express toward the political system. The civil culture data are more or less consistent with the authors’ theory. However there are important unanswered questions that remain from this and other studies using individual-level survey data to discuss state-level political patterns that arise from the subjective, but not intersubjective, nature of survey data, such as the ways opinions are aggregated to produce collective action and the lack of identification of mechanisms to explain the cross-level linkage (Berman 2001: 241). While survey data could certainly be employed to help us understand meaning and meaning-making, the work in this area rarely does this.

Culture frames the context in which politics occurs. Culture orders political priorities (Laitin 1986: 11), meaning that it defines the symbolic and material objects people consider valuable and worth fighting over, the contexts in which such disputes occur, the rules (both formal and informal) by which politics takes place, and who participates in it. In doing so, culture defines interests and how they are to be pursued. Cultural understandings are at the core of the definition of political communities since people in a community share, in Geertz’s (1973c) words, “schematic images of the social order.” Authority in any political

community is culturally constituted, and its establishment and maintenance are often explained in cognitive and cost-benefit terms as well as in terms of threats of coercion, but it also involves ritual activity that connects people's everyday experience and anxiety to those of the collectivity (Edelman 1964; Kertzer 1988; Turner 1957, 1968).

Culture links individual and collective identities. Culture offers emotionally significant connections between the fate of individuals and the group. The crucial connection at work is that of identification, which renders the connection seamless and removes alternatives that on other grounds might be equally plausible. Individual and collective action, this view suggests, are motivated, in part, by the sense of a common fate people in a culture share and involves two distinct elements: the strong reinforcement between individual and collective identity that renders culturally sanctioned behavior rewarding and the sense that outsiders will treat oneself and other members of one's group in similar ways. In addition, the dynamics of within-group identity formation overemphasize what it is that group members actually share and gives greater emotional weight to the common elements especially in periods of high stress, reinforcing their strong sense of within-group linked fate.

Culture defines group boundaries and organizes actions within and between them. Although cultures are not formally bounded entities, culture nonetheless defines identity groups, and in the process specifies expectations concerning patterns of association within and between them. Cultural definitions of social groups – whether they are defined by kinship, age, gender, or common interests – entail clear expectations about how people are to act, even when these definitions are continually contested (Greif 1994; Scott 1985). How such social categories and groups are defined and the norms that regulate their behavior vary cross-culturally. Cultural norms regarding relations between groups in the same culture can be highly elaborate. While cultures differ in how and when they restrict, as well as how they enforce restrictions on, relations, few are silent on these questions.

Culture provides a framework for interpreting the actions and motives of others. Actions, like words, are highly ambiguous, and making sense of them requires a shared cultural framework to ensure that the message sent is similar to, if not identical to, the one received. Few behaviors are so universal that they require little or no interpretation and invocation of culturally available narratives and scripts to help people make sense of ambiguous but emotionally salient situations (Petersen 2005; Ross 1997). Motives are central to cultural analysis because they offer a mechanism to link individual action to a broader social setting (D'Andrade and Strauss 1992).⁸ In many ways, motives in cultural analysis are much like interests in rational choice theory. In statements such as "They were motivated by fear of their ancestors, so they sacrificed half of their livestock" or "Country X had an interest in weakening the military capability of its enemy,"

⁸ This contrasts with Geertz's focus on "inspecting events" and making sense of actors' interpretations of them but his rejection of the idea that we should examine mental structures (1973a: 10–12).

both motives and interests offer a “reasonable” account of why individuals or groups behave in a certain way. Yet, there are also significant differences in the use of motives and interests as explanatory mechanisms that are central to the difference between cultural and rational choice explanations. While interests are assumed to be more or less transparent (some would say given) and universal, motives are knowable only through empirical analysis of particular cultural contexts. As a result, while turning to interests suggests that almost any human group would behave the same way in a certain situation, an emphasis on motives focuses on explaining variation in behavior across groups (Wildavsky 1987). Cultural explanations, as a result, do not deny the relevance of interests but see them as contextually defined and as but one motive among many.

Culture provides resources for political organization and mobilization. Culture offers significant resources that leaders and groups use as instruments of organization and mobilization (Brysk 1995; Edelman 1964; Kertzer 1988; Laitin 1986; Ross 2007). We can think of action-oriented scripts that can be mobilized (Petersen 2005) or what Tilly calls “repertoires of collective action,” referring to “a limited set of routines that are learned, shared, and acted out through a relatively deliberate process of choices. Repertoires are learned cultural creations” (Tilly 1995a: 26; Tilly 1986; Traugott 1995; McAdam, Tarrow, and Tilly this volume). In his formulation, anthropologist Abner Cohen (1969, 1974, 1981, 1993) spells out more generally the political uses of culture, emphasizing the importance of cultural organizations (formal or informal groups organized around specific cultural practices) and how they use cultural organizations to pursue goals that cannot be pursued directly (Cohen 1969: 201–210).

In discussing religion, the prototypical cultural basis for political organization, Cohen points out that

[r]eligion provides an ideal “blueprint” for the development of an informal political organization. It mobilizes many of the most powerful emotions which are associated with the basic problems of human existence and gives legitimacy and stability to political arrangements by representing these as parts of the system of the universe. It makes it possible to mobilize the power of symbols and the power inherent in the ritual relationship between various ritual positions within the organization of the cult. It makes it possible to use the arrangements for financing and administering places of worship and associated places for welfare, education, and social activities of various sorts, to use these in developing the organization and administration of political functions. Religion also provides frequent and regular meetings in congregations, where in the course of ritual activities, a great deal of informal interaction takes place, information is communicated, and general problems are formulated and discussed. The system of myths and symbols which religion provides is capable of being continuously interpreted and reinterpreted in order to accommodate it to changing economic, political and other social circumstances. (1969: 210)

We must continue to ask why and how the appeals to religious, national, and ethnic identities are so powerful and mobilizable. As Campbell (1983) has suggested, any answer that relies on mechanisms of individual benefit only make sense if we can also account for the strength of individual attachments to groups such as those defined in cultural terms.

THE CENTRALITY OF PSYCHOCULTURAL NARRATIVES
AND INTERPRETATIONS IN CULTURAL ANALYSES OF POLITICS

Cultural analyses are among the oldest works of political science and have taken different forms over time.⁹ Here I simply note a few of their features and then advance what I see as a much more potentially productive way to conduct cultural analysis of politics. In some older works, culture resembles race and is viewed as having a strong biological component. In these, culture is often defined in terms of key distinguishing traits, values, or behaviors and is described in single terms such as “fierce,” “intelligent,” “warlike,” “freedom-loving,” “pious,” “responsible,” “traditional,” or “manipulative.” This work has several problems: It is often not clear what the evidence is for the overgeneralizations about large collectivities; there is scant attention to how these characteristics were produced and reproduced; and these traits are often used very mechanically to explain collective behavior.

More recent works, particularly those associated with political culture studies, often adopt a reductionist framework to explain aggregate-level outcomes and differences in terms of variations across systems in the distribution of individual attitudes, values, and behaviors. All too often, however, there is little concern with the mechanisms that might link these individual-level differences to the collective behaviors of interest. As a result, the connections are often treated as self-evident rather than demonstrated, and alternative ones are not considered. In much of this work the concept of culture itself is undertheorized, and operationally it is overly reliant upon disaggregated, often single, indicators. Finally, too little effort is made to examine actions and meanings from a holistic perspective, resulting in very thin explanations of politics (Fearon and Wendt 2003) and creating little sense of how and why culture is regularly contested and renegotiated.

A better alternative is found in contemporary intersubjective approaches to culture and politics that emphasize meaning and meaning-making and the concept of interpretation that problematizes how political actors understand their world, as well as the relevance of their understanding of it for political action. This perspective is especially interested in linking micro-level frameworks and events to macro-level outcomes via the shared interpretations through which people make meaning from the ambiguous and fragmented elements of daily life (Darnton 1985; Taylor 1985) and the narrative accounts that articulate widely shared understandings about individuals, groups, their motives, and past and contemporary events. In the first edition of this volume, I mainly discussed interpretations and hardly mentioned narratives. Since that time, I have come to distinguish between the two more clearly, seeing interpretations as people’s underlying beliefs, emotions, and worldviews and narratives as the more accessible verbal and nonverbal expression of these underlying elements (Ross 2007). Psychocultural¹⁰ interpretations, then,

⁹ In my earlier chapter (Ross 1997), I discussed some of these approaches.

¹⁰ I use the term “psychocultural interpretations” to describe how shared interpretations are acquired through individual-level psychological (and social-psychological) mechanisms that are

are the building blocks for psychocultural narratives that are more readily accessible and use common everyday shared understandings to offer plausible accounts of the world.

Psychocultural Narratives and Interpretations

Psychocultural narratives are socially constructed explanations for events – large and small – in the form of short commonsense accounts (stories) that often seem simple to outsiders.¹¹ The evocative images they contain, and the judgments they make about the motivations and actions of one's own group and those of opponents, are readily available and emotionally powerful. The images are found throughout a society's symbolic landscape and communicate many specific emotions and positions, including intergroup power relations and messages about inclusion and exclusion. Narratives explain past, present, and future events in emotionally meaningful ways that render alternative action possibilities more or less plausible. In examining narratives, one should be less concerned with their truth or falsity than with their emotional power and plausibility in specific political contexts.

Narratives are not always internally consistent, and in-group disagreement about parts of a narrative and its meaning can produce divergent reactions (Aronoff 2001; Wedeen 2002). Cultural analyses of politics are especially interested in situations where there is contestation among competing narratives (e.g., differences between religious and secular Jews in Israel or between Israeli Jews and Palestinians). Why and how one narrative comes to achieve dominance over another for a time is an important question that cultural analyses address. Narratives are best understood as existing at different levels of generality in which consensus is invariably greater on the general themes than on more specific details, and various elements – especially those with more specificity – are regularly added, discarded, rearranged, emphasized, and deemphasized. All cultural traditions have access to multiple preexisting narratives that provide support for a range of plausible actions in anxious times, and when dominant narratives develop, it is clear that they are not made from whole cloth, but are grounded in selectively remembered, interpreted experiences and projections from them that resonate widely in a group, as Lustick (2006) argues in his analysis of the U.S. “war on terror.” Finally, narratives are renegotiated and changed in subtle or even major ways as contexts change.

widespread in a culture. The term “psychocultural” brings together the psychological processes central to the construction of these interpretations and cultural dynamics, emphasizing that these orientations are not just personal, but rather are nurtured and socially reinforced, linking individuals in a collective process, amplifying what is shared, and emphasizing differences among groups. The same dynamic is relevant to understanding psychocultural narratives as well (Ross 1993a, 2007).

¹¹ For a more extended discussion of psychocultural narratives, their key features, and their use in the analysis of cultural contestation, see Ross (2002: chap. 2; 2007).

Psychocultural narratives have several key features that I have found especially useful for studying the politics of cultural contestation (Ross 2007: chap. 2). First, they are selective accounts, drawing on key images and events from a group's collective memories as they are relevant to contemporary situations. In narratives, past events serve as (often timeless) metaphors and lessons that guide present and future action. Politically relevant narratives contain both accounts of collective fears and threats to identity, as well as recalling past heroic deeds and triumphs (Volkan 1997). Many collective accounts are ethnocentric, adopting a morally superior stance and demanding high in-group conformity in periods of stress while externalizing responsibility for a group's present troubles. Narratives evolve, particular scripts in them gain or lose their emotional intensity, and their elements take on new relationships to each other as contexts and relationships change. A good example of this evolution is seen in the increased importance of Jerusalem and its holy sites in both Israeli Jewish and Palestinian narratives in the past 120 years. It is not that current narratives have no older roots – for they do – but rather that there was a marked shift in the context as each group aggressively asserted its rights and denied those of the other.

Psychocultural narratives play three different substantive roles in political life. They can be, and are most often seen as, *reflectors* of deeply held cultural understandings and beliefs. Examples include textbook accounts of a nation's history or national anthem. Second, they can be examined as *exacerbators* or *inhibitors* of within- or between-group differences in ways that variously escalate or deescalate conflict. One example is found in Lustick's (2006) interpretation of the spate of novels, films, and television shows in the United States that reinforced the fear of terrorism and the need for a "war on terror" to make the country safe following the September 11 attacks. Third, they can serve as *causes* of action, not because ideas get people to act directly but because they frame action alternatives such that certain options get serious consideration while others are virtually ignored. For example, following September 11, the Bush administration successfully framed the attacks as terrorist acts, while the 1994 attacks on the World Trade Center were treated as criminal actions. Once they were framed this way, the government actions considered appropriate included going to war in Afghanistan and Iraq and altering civil liberties and privacy rights. Of course, these three categories are not mutually exclusive, and any narrative may serve more than one function.

Conceptually and methodologically, interpretations and the narratives through which they are communicated are a key tool to examine culture and politics. The rich accounts found in the images of the world people offer point to intense concerns, assumptions about the organization of social and political relations, and the possibilities for political action. These images are obtainable, in part, through public and private accounts. However, simply presenting the transcripts of individuals' stories is insufficient, for texts do not speak for themselves and often make little sense to an outsider (Scott 1985: 138–141). It is difficult to derive much significance from detailed, highly contextualized

interpretations whose meanings are often not self-evident (Chabal and Daloz 2006). Laitin (1988) says that connecting culture and action requires detailed local ethnographic knowledge and experience for a rich contextual understanding.¹²

The interpretations and narratives of particular interest here are accounts of the world that people within a culture widely share in the sense that they are mutually understood. When supported by one's social world, these plausible accounts offer psychic and social protection from the ambiguities and uncertainties of existence, reinforcing social and political bonds within groups. The power of psychocultural interpretations and narratives lies in their shared social character, not in those idiosyncratic features that distinguish one person's account from another's. As Taylor writes, "they are not subjective meanings, the property of one or some individuals, but rather inter-subjective meanings which are constitutive of the social matrix in which individuals find themselves and act" (1985: 36).

At the core of psychocultural interpretations are internalized, shared orientations rooted in the earliest social relationships that help people in a culture make sense of inherently ambiguous, highly charged events that characterize their lives (Ross 1993a). Psychocultural interpretations draw attention not just to what people do to each other but also to what one group of people think or feel that another group of people is doing, trying to do, or wanting to do. In a context of suspicion and uncertainty, not only actions, but also presumptions about the intentions and meanings behind the actions (or inactions), play an important role. This is crucial, for in few political situations do external events provide clear explanations for what is occurring; to develop these, individuals turn to internal frameworks and groups turn to their collective memories, which then shape subsequent behavior. While participants in any dispute can often tell someone "just what the conflict is about," this precision is often illusory, so political scientists too hastily see this as evidence of flawed decision making and/or faulty information processing. However, it is more useful to view these "errors" as important data about social dynamics.

In many situations, different parties don't agree on what a conflict is about, when it started, or who is involved, for they operate from (but are not aware of) alternative frames of reference. Many disputes, whether they are between families in a community or the world's states, involve parties with a deep history, which, of course, includes long lists of accumulated grievances that can be trotted out and appended to newer ones as political conditions shift (Scott 1985). Historical memory and the symbols evoked in its recounting become emotionally important as alternative events are invoked by each side or the same event is interpreted in very different ways.

The same factors that push actors to make sense of a situation also lead to cognitive and perceptual distortions because the desire for certainty is often

¹² This is the same claim that many students of the U.S. Congress have made over the years.

greater than the capacity for accuracy. Not only are disputants likely to make systematic errors about the “facts” underlying interpretations, but the homogeneous nature of most social settings and cultural amplifiers reinforces these self-serving mistakes. What is most crucial, however, about politically relevant narratives and interpretations is the compelling, coherent account they offer to the parties in linking discrete events to general understandings. Central to such interpretations is the attribution of motives to parties. Once identified, the existence of such motives seemingly makes it easy to “predict” another’s future actions and, through one’s own behavior, to turn such predictions into self-fulfilling prophecies. For this reason, it is appropriate to suggest that rather than thinking about particular objective events that cause conflicts to escalate, we ought to be thinking about the interpretations of such events that are associated with escalation and those that are not.

This is seen vividly in stories about long-standing ethnic conflicts that contain the culturally rooted aspirations, challenges, and deepest fears of communities. Volkan (1997) develops the term “chosen trauma” to refer to a specific experience that comes to symbolize a group’s deepest threats and fears through feelings of helplessness and victimization. He provides many examples of such events, including the Turkish slaughter of Armenians, the Nazi Holocaust, the experience of slavery and segregation of African Americans, and the Serbian defeat at Kosovo by the Turks in 1389.¹³ If a group feels too humiliated, angry, or helpless to mourn the losses suffered in the trauma, Volkan suggests that it then incorporates the emotional meaning of the traumatic event into its identity and passes on the emotional and symbolic meaning from generation to generation. In escalating intergroup conflicts, the key metaphors, such as the chosen trauma, serve both as a rallying point and as a way to make sense of events that evoke deep fears and threats to existence (Horowitz 1985; Kelman 1987). Only when the deep-seated threats these stories represent are addressed, Volkan suggests, is a community able to begin to conceptualize a more peaceful future with its enemies.

Cultural Expressions and Enactments

Starting from a perspective that views culture as a template that people use to make sense of the world, we should be especially attentive to ways in which these templates are linked to cultural expressions and enactments that mobilize in-groups and out-groups through the invocation of contrasting and polarizing responses (Ross 2007). Many expressions and enactments consist of displaying mundane objects or performing behaviors such as language use (think Quebec, the Basque country) or clothing (think about Islamic headscarves in Europe).

¹³ See Volkan (1997) for a discussion of what he calls “chosen traumas” and of the relevance of this concept for understanding long-term ethnic conflicts. The flip side is the chosen glory in which a group perceives triumph over the enemy; this is seen clearly in the Northern Irish Protestant celebration of the Battle of the Boyne in 1689 every July 12 and in the Independence Day celebrations in many countries.

Others involve single events such as the publication of the Mohammed cartoons in Denmark. Finally, there are regular, but not daily or ordinary expressions such as the Japanese prime minister's yearly visits to the shrine that includes the remains of some of the country's World War II war criminals, Ku Klux Klan rallies in the United States, Orange Order parades in Northern Ireland, or Hindu nationalist mobilization calling for the (re)construction of a temple to Rama in Ayodhya. Examining such expressions and enactments is a tool to get at deeper levels of meaning for groups and to better understand the fears, hopes, and worldviews of those involved in social interactions ranging from cooperation to contestation.

Cultural expressions and enactments connect group narratives and identity in many ways. Public ceremonials, religious ceremonies, calendrical festivals and holidays, theatrical presentations, television programs, literature, public discourse, distinctive clothing and foods, and language use are some of the common ways that narratives – or key parts of them – are enacted, communicating and reinforcing in-group understandings. Merelman's (1991) comparative study of political culture makes good use of diverse forms of expression including television programs, corporate publications, textbooks, and magazine ads, along with survey data as data sources in his comparative examination of the United States, Canada, and Britain. In another interesting example using a cultural indicator of collective political orientations, Regan (1994) relates sales of war toys and the popularity of war movies to U.S. militarization. More recently, Croft (2006) provides a detailed analysis of American popular culture, including films, television programs, and human and political discourse, to explain how the war on terror has become naturalized and used to promote the Bush administration's domestic and foreign policy aims.

In addition to considering single expressions or enactments, we can examine a society's symbolic landscape to ask how it communicates social and political inclusion and exclusion (Ross 2007, 2008). The symbolic landscape consists of the public images found in physical objects and other expressive representations in public spaces – especially sacred sites (which are not necessarily religious ones) – and other emotionally important and visible venues, as well as group representations in the mass media, theater, school textbooks, music, literature, museums and monuments, and public art. Symbolic landscapes reflect how people perceive and experience their world and others in it, but they can also be significant shapers of these worlds when they establish and legitimate particular normative standards and power relations within and between groups (Cosgrove 1998). Symbolic landscapes communicate inclusion, exclusion, and hierarchy, and portray dominant and subordinate groups in particular ways. The meanings a symbolic landscape conveys and the narratives it invokes invite us to ask: Who is present and who is absent in public representations? What are the qualities of the people and objects portrayed in them? Who controls the representations, and to what extent are they contested? How is hierarchy portrayed, and what qualities are associated with particular positions within a society's hierarchy?

Inclusion and exclusion are often powerfully expressed through the restriction or expansion of a society's symbolic landscape. Exclusion of groups from the symbolic landscape is an explicit form of denial and an assertion of power. In contrast, a more inclusive symbolic landscape can be an expression of societal inclusion that communicates mutuality and a shared stake in society. It renders the previously unseen seen, gives voice to those once voiceless, and can offer powerful messages to reshape relations between groups. Through inclusion, groups can identify more easily, and can help mourn past losses and express hopes and aspirations for a common future. As symbolic statements of acknowledgment, it is no wonder that some public sites and the representations they contain can become the source of intense controversy, raising issues such as: What stories do they choose to tell about themselves? How is this related to who can speak for the group? Who controls its narrative and the images associated with it (Linenthal 1993, 2001)?

Culture can be part of an explanation for many political phenomena. In analyses of conflict, for example, we see how narratives evolve as a result of escalation reinforcing and hardening in-group worldviews, and in-group solidarity and out-group hostility both increase. Not all cultural mobilization in conflict situations leads to conflict and violence, however, and one interesting comparative question asks when this turn to violence does and does not occur. For example, Laitin (1995) offers a controlled comparison emphasizing culturally constituted differences in social organization to explain the frequent use of political violence in the Basque protest in Spain and its relative absence in nearby Catalonia. As a result, the differences in the use of violence between the regions are not a function of objective differences or their relative deprivation, but of the cultural organization of each community, which affects the likelihood that the ethnic revivals in each one will turn to violence.

Raymond Cohen (1990, 1991) shows that culture can frame how political leaders understand the actions and words of those from other states and how such cultural assumptions can complicate the task of diplomatic negotiations in his examination of Egyptian–Israeli negotiations over time (1990). In a second study, he details how cultural miscommunication has affected U.S. negotiations with Mexico, Egypt, China, India, and Japan, arguing that differences in time frames, the importance of context, language, and an individualistic versus collectivist ethos all are important in either inhibiting or facilitating negotiated efforts to deal with substantive issues. Focusing only on “substance” and ignoring culture, he contends, leads to serious missed opportunities and failures where successes might have emerged.

Interpretations and Narratives as Methodological Tools

If, as Freud suggested, dreams are the royal road to the unconscious, in cultural analyses of political events, psychocultural narratives and interpretations are key methodological tools. They begin with people's accounts of their daily worlds, and comparative researchers of all persuasions easily recognize many of the

forms in which such accounts appear, such as formal written materials, historical documents, public discourse, government records, law cases, systematic observations, and survey data. In addition, the rich accounts often needed in cultural analysis can only be obtained through ethnographic field research; in-depth interviews and life histories; structured interviews; extended case analysis of trouble cases; and cultural expressions and enactments. Certainly the process analysis that Migdal, and McAdam, Tarrow, and Tilly advocate in their chapters in this volume is central to cultural analysis, with its emphasis on interpretations.

Analyzing narratives and interpretations over time is a tool for understanding the contested nature of history and for discerning how one account comes to be accepted as “what really happened” while other plausible stories are rejected. Interpretations of the past are found in how people talk and write about it, but are also found in the public rituals and myths built around key events in the national or ethnic past and in public performances that are not always explicitly political. The rituals and myths are significant because of the meanings and metaphors surrounding a group’s history they communicate and reinforce emotionally, and because of the political mobilizing potential they have in the hands of political entrepreneurs.

The central goal of cultural analysis from a meaning and identity perspective is to understand, from the worldviews of actors in a particular context, why certain actions are undertaken and others are not. What this entails is asking why and how people then adopt particular worldviews and how their specific scripts become important in political mobilization or quiescence. Bridging the gap between a researcher’s understanding of a particular context and the elements in local actors’ narratives, and the connections between them, requires calling on cultural understandings and working to develop mediating explanations that make sense to cultural insiders and are seen as plausible by outsiders.

Examining rituals is critical to understanding how most people construct and understand political reality (Edelman 1964, 1988; Kertzer 1988: 77–101). Political rituals offer meaning in ambiguous, uncertain situations and are crucial to the dynamics of identity construction and maintenance, particularly in periods of change. In bringing certain people together, culturally rooted rituals and the scripts underlying them simultaneously exclude others. Powerful political rituals are those that utilize culturally powerful symbols, metaphors, and meanings to create and structure perceptions of reality; often this involves pitting one group against another by raising fears and threats to the point that people are all too ready to undertake strong action in the name of the group. The power to control ritual is important for the following reason:

Indeed, ritual is an important means of influencing people’s ideas about political events, political policies, political systems, and political leaders. Through ritual, people develop their ideas about what are appropriate political institutions, what are appropriate qualities in political leaders, and how well the world around them measures up against these standards. (Kertzer 1988: 79)

Spelling out how culture renders specific symbols and rituals significant in a particular context, and how they both create and reinforce political identities, is central to this dynamic (Brysk 1995; Kertzer 1988). For example, whereas some analyses emphasize elections as citizen choice-making, a focus on symbol and ritual draws attention to how political parties and candidates in their quest for power use culturally shared metaphors and fears in their appeals to citizens and to build compliance among them (Wedeen 1999). Policy positions or candidate choices, we now understand, are not just about the issue preferences of individuals; we also must ask how such orientations are or are not consonant with shared cultural understandings and identities. Invocation of symbols and use of rituals do not just indicate points of consensus; they are also efforts to overcome contradictions in situations of disjunction (Kurtz 1991: 149). This richer understanding of the cultural roots of politics has produced an interest in how political rituals create (rather than just reflect) meanings and shape actions (Gusfield 1966). In short, rituals frame issues that establish compelling priorities. In doing this, they are important instruments of control and, from a Gramscian perspective, are central mechanisms for obtaining and maintaining power. Through analyses of meaning construction and meaning-reinforcing rituals, we can examine social movements as well as the mass media, which provide not only access to core knowledge but also the frameworks for making sense of it to citizens of mass democracies (Dayan and Katz 1992).

With a few rare exceptions, the most successful work linking culture and politics will not rely on only one source of data or a single tool for data analysis. Our most interesting theories are complex and highly contingent, and cannot be simply accepted or rejected on the basis of one crucial piece of evidence. Instead, we need to obtain areas of convergence between independent data collected using a wide range of methods to have confidence in a set of findings, as Campbell and Fiske (1959) advocated long ago. Exclusive reliance on one type of data to study the interplay of culture and politics, as is found in some survey researchers such as Inglehart (1988), inevitably produces a thin, almost content-free sense of culture and points to few dynamics of how culture produces the political effects it does.¹⁴ Instead, a more useful approach is found in Scott (1985), Laitin (1995), Petersen (2002), and Bowen (2007), in which a range of evidence is marshaled to explain phenomena that are not self-evident: the presence of everyday, but not overt, resistance among Third World peasants; why in Spain the Basque revival has been violent, while under very similar circumstances, the Catalan one has not; the shifting choice of targets of ethnic violence; and why France came to pass a law in 2004 banning Islamic headscarves in public schools.

Two Examples. Political scientists write about narratives more than ever these days in an effort to describe and analyze micro-level events. The concept has

¹⁴ My critique is on the aggregative nature of cultural explanation as a cluster of attitudes built from the survey data employed. Jackman and Miller (1996), in a reanalysis of the data, suggest that even the empirical claims Inglehart makes are unwarranted.

even made its way into rational choice analyses (Bates, de Figueiredo, and Weingast 1998), albeit with an interest in analytic narratives aimed at providing a coherent analytic explanation for events rather than capturing the contextually grounded worldviews and motivations of local actors. Here, it is probably useful to describe two very different political analyses that give a central role to culturally rooted narratives in their explanations for particular outcomes.

Roger Petersen (2002) seeks to explain the targets and the intensity of violence across the twentieth century in Eastern and Central Europe.¹⁵ His account is rooted in the emotional activation of pressing concerns through the invocation of a specific historically and contextually rooted narrative. For each of the emotions he considers – fear, hatred, resentment, and rage – Petersen derives specific predictions about who the local targets of violence are most likely to be, and he generates competing predictions to be able to evaluate the relative explanatory power of each emotion. The emotions each narrative activates serve as the linkage mechanism between micro-level and macro-level collective action. “Each emotion-based narrative provides an explanation of how, in the face of social complexity and fluidity, such a brutal simplicity comes to frame outlooks and motivate action” (Petersen 2002: 3). In the end, he finds that while the resentment narrative, centered on a belief and a sense of unjust group status, provides the best predictive and descriptive fit for a variety of cases of ethnic conflict in Eastern Europe, he also finds that there are situations when fear, hatred, and rage seem to operate as well.

The power of Petersen’s book is his ability to articulate specific narratives associated with distinct emotions and then to suggest ways that each might have operated in specific ethnically based violence in the region at different periods of time. His ability to identify competing hypotheses and to make specific testable predictions is rare and powerful. The mechanisms are explained in terms of social psychological theories, while the narratives are deeply rooted in the historical and cultural experiences of the region, providing a powerful example of how psychocultural narratives are deeply implicated in ethnic interaction and actions.

John Bowen (2007) asks a simple question: How was it that in 2004 France came to pass legislation that banned the wearing of Islamic headscarves (and other religious symbols) in schools? He also puzzled about how a piece of “cloth came to stand for certain fears and threats” that produced an intense conflict (2007: 4). Bowen’s analysis emphasizes the power of the dominant French Republican narrative to frame the debate and the specific options that were and were not actively considered. He aptly describes his project as an “anthropology of public reasoning” in which he connects political philosophy, public policy, and common sense, asking how people deliberate on an important social issue that connects micro-events to macro-level outcomes. To do this, Bowen weaves

¹⁵ See Petersen for another case in which he skillfully uses data on how a specific event unfolded to explore the impact of memory and cultural schema – an abstract mental model containing specific scripts – “by trying to establish the foundations of cultural schemas in the actual experiences and memories of individuals and communities” (2005: 133).

together the narrative about how the French understand the common good, the importance of the nondenominational state as the protector of its citizens and the guarantor of order and liberty, and the threats that intermediate identity groups and public religious practices represent to France. In this widely shared narrative, the public school is the key tool for inculcating a common understanding of society and for creating citizens who are not divided into distinguishable and unchangeable identity groups.

As the conflict intensified, the “reasonableness” of the proposed legislation dominated political talk to the majority of those on the left and right and in media coverage, so that there were never really questions about whether the law would have any impact on the deeper underlying issues such as unemployment, inequality, segregation, and discrimination that were the real sources of ethnic tension in France. In the process, there was little or no interest in what the people wearing the headscarves had to say about their reasons for doing so; instead, their actions were simply widely criticized for promoting communalism, Islamism, and sexism, each of which was seen as a strong threat to key French values and practices. The Islamic voices that the French heard (and wanted to hear) were those of “reasonable” (meaning not practicing) women and men who opposed headscarves and favored the law banning them. In making his argument, Bowen provides a rich micro-level account of French politics, competition and differences among Islamic organizations in France, media coverage, the official hearings, and parliamentary debates.

In the end, the imposition of the powerful narrative framed the conflict so that “what ensued were tempestuous debates about what *laïcité* *should* be and how Muslims *ought* to act, not in light of a firm legal and cultural framework, but in light of a disappearing sense of certitude about what France was, is, and will be. Hence the desperation; hence the urgency” (Bowen 2007: 33). Whose voices were heard mattered because they defined and moved the political actions leading to the eventual outcome. What Bowen does especially effectively is to offer a specific, well-developed explanation of how a widely shared psycho-cultural narrative plays a critical role in shaping understandings and actions in the course of a long-term conflict. He makes it clear that the outcome was not inevitable, that there were at key points differences in interpretation and clashing views, key actors whose framing and actions moved the conflict in a particular direction, dramatic media events that crystallized public opinion and political positions, and exogenous events such as events in Algeria, 9/11, and the second Intifada that further affect the conflict in the French context, making specific actions more or less likely than they were earlier.

CRITIQUE OF CULTURAL ANALYSES OF POLITICS

Cultural studies of politics have been subject to a number of strong criticisms that deserve consideration. Perhaps the most significant problems arise over methodological issues such as the lack of precision about culture as a unit of analysis and the question of within- versus between-cultural variations. Others are concerned with the vagueness of the concept of culture and the difficulty

of distinguishing culture from related concepts such as social organization, political behavior, and values. Some are worried that since culture suggests relatively fixed, unchanging patterns of behavior, it is not useful in accounting for changes in behavior and beliefs. Finally, there is concern that cultural analyses are not sufficiently explicit concerning the mechanisms linking culture and political action. Each of these concerns is worth some attention. However, it should be clear that my view is that none of these problems is fatal and that in cultural approaches to comparative politics, like those rooted in interests or institutions, the best way to address the conceptual and methodological problems is through a multimethod search for convergence rather than abandonment of the paradigm.

Unit-of-Analysis Issues

Defining the unit of analysis precisely – be it voters, states, wars, or international organizations – is one of the first lessons of most methods seminars in political science. “What is a culture?” some political scientists ask, meaning, “How do I know one when I see it?” since culture is not a unit of social or political organization with readily identifiable boundaries. Furthermore, the imprecision of common language use makes it very unclear what are a culture’s borders. As a result, we hear references to Western culture, French culture, Breton culture, rural Breton culture, and so on. Where does the parsing stop?

The unit-of-analysis problem is about what constitutes the core of a culture and also how to identify its edges (Barth 1969). Where does one culture stop and another begin? Since cultures, unlike states or political parties, are not formal units, treating them as independent units of political analysis can be troubling indeed. While this may seem like a devastating critique, it is just as true that neither states nor voters are truly independent units either, although most cross-nation and survey analyses assume they are. What is probably more useful is to be more sophisticated about interaction effects and the influence of one unit upon another.¹⁶

Conceptually, the best answer is that the appropriate level of analysis depends on what one wants to explain. Horowitz (1985) says that which level of cultural identity is the most salient at any moment depends upon where someone is and what he or she is doing with whom. However, this is not always an easy-to-use methodological guide in empirical research. Probably the best answer to this methodological question is to begin with the recognition that cultural identity is layered and situationally defined.¹⁷ People hold multiple identities, some

¹⁶ Galton’s Problem refers to the fact that in cross-cultural (and cross-national) samples, assumptions about the independence of units are often inappropriate and that substantive correlations among culture traits can reflect diffusion and borrowing rather than functional association. The most useful response is not to ignore this problem, but to build diffusion hypotheses into models to test the relative power of each pattern (Ross and Homer 1976).

¹⁷ Another answer comes from Thompson, Ellis, and Wildavsky (1990), who argue that culture is seen in distinct ways of life that they define in terms of Mary Douglas’s grid-group analysis. “Group” refers to the extent to which an individual is incorporated into bounded units, while “grid” refers to the degree to which a person’s behaviors are circumscribed by externally imposed restrictions. Different individuals or states can, in their view, exhibit different degrees of each of

identities partially overlap, and the boundaries can shift across issues or contexts. Despite the methodological problems this can present, we cannot ignore culture if we think it is important, so we should make decisions about units of analysis based on what we are trying to explain rather than on abstract criteria intended to identify a set of cultural units akin to a list of all UN member states.¹⁸ In addition, there are a number of other procedures we can use to define cultural units in particular pieces of research. For example, we can use operational criteria such as asking people how they identify themselves and others, and we can use social consensus about particular cultural groupings and their boundaries. The point is that a task for research is to identify relevant groupings in whatever situation is under study. The fact that people possess multiple identities or that identities can change over time does not invalidate such analysis; it just makes the research more complicated.

Political culture research in the Almond and Verba tradition sought to solve the problem by defining culture as the aggregate of individual orientations within political units. Almond and Powell (1966), for example, define political culture as “the pattern of individual attitudes and orientations towards politics among members of a political system” (1966: 23). Reducing culture to the sum of individual attitudes is hardly adequate, however, since it ignores both the context in which particular attitudes are held and the shared understandings that organize clusters of intersubjective orientations (Merelman 1991). Culture, as a result, is not about meaning, meaning-making, and identity; rather, it is simply a frequency distribution on a set of single-attitude items – a kind of machine with totally interchangeable parts. While studying individuals is part of how we can understand culture, as both Taylor (1985: 37) and Geertz (1973a) argue, culture is not the property of single individuals. Rather, it is an emergent property rooted in social practices and shared understandings that cannot be uncovered through survey data alone. For this reason, individual-level survey data are but one tool for studying political culture. They must be used in conjunction with other data to provide a rich, coherent portrait of any single culture or comparisons between cultures.

Within-Culture Variation Can Be Substantial

It often seems easy to say what members of formal groups have in common. But what exactly do people of a given culture share? My answer emphasizes shared worldviews – but not necessarily particular values or beliefs – and a common

the five combinations they identify over time. However, viable social units, they argue, are not characterized by the presence of only one culturally defined way of life. While I find much of their analysis of the interaction between values and social structure quite useful, it is less evident to me that making the way of life the unit of analysis provides a guideline that is easy for researchers to use, since they say that multiple orientations can exist in the same culture or subculture and that individuals may have different orientations across time and situations.

¹⁸ This does not mean that such a list of societies that represent the world’s cultures, such as one of the samples developed in cross-cultural research, is not useful in many situations.

identity. Operationally, this can be ambiguous, however, for we know that people who themselves identify as members of a culture (or any organized group) are likely to differ in terms of values, lifestyles, political dispositions, religious beliefs and practices, and ideas about common interests. In addition, Strauss cautions that while there may be some variation in schemas (what I have been calling worldviews) across individuals in the same culture, even those with very similar schemas may not internalize exactly the same things, and that the ambiguity of metaphor produces variation in responses (Strauss 1992: 10–11). In short, there is also within-culture diversity (Norton 2004). However, this is not necessarily any more of a problem in dealing with culture than it is in dealing with other units (or even with intraindividual variation in behavior or attitudes over time). LeVine argues that emphasizing culture as common understandings of the symbols and representations they communicate does not mean that there is necessarily a problem with within-cultural variation in thought, feeling, and behavior (LeVine 1984: 68).

A second answer is that often what is more crucial politically than agreement on content is that people share a common identity, although this still leaves open the question of different degrees of identification and differences in the actions people are willing to undertake in the name of that identity. Shared identities mean that people see themselves as similar to some people, different from others, and open to potential mobilization on the basis of these differences. What an emphasis on identity stresses, once again, is that the relevant critical aspects of cultural similarity and difference are defined in specific political contexts. It is also the case that often what people believe they share may be at odds with reality because perceptions of cultural homogeneity overemphasize what is actually shared, minimizing within-group differences while stressing between-group differences. In this dynamic, in-group conformity pressures will lead people both to selectively perceive greater within-group homogeneity on critical characteristics than actually exists and to generate greater actual homogeneity and group conformity in situations where perceived threats to the culture are great.

Distinguishing Culture from Other Concepts

Some uses of culture, such as much of the early work on national character, defined culture so broadly as to include society, personality, values, and institutions. In fact, nothing was really excluded. The very broad use of the concept of culture is also found among social scientists emphasizing culture as a source of the social integration of a society. This perspective, probably clearest in functional theory in British social anthropology, uses culture to refer both to distinct elements of social organization and to the “fit” between different parts of a cultural system and the integration of the whole. The problem here is not the concept of culture but the way it is used. As noted previously, the focus on culture as meaning and meaning-making is distinct from social structure and behavior. D’Andrade makes this particularly clear in his description of culture “as consisting of [a] learned system of meaning, communicated by means of

natural language and other symbol systems, having representational, directive, and affective function, and capable of creating cultural entities and particular senses of reality. Through these systems of meaning, groups of people adapt to their environment and structure interpersonal activities” (1984: 116). In addition, Spiro’s (1984) clear distinction between culture as a system of meaning and what he calls “culturally constituted elements,” referring to social structure, behaviors, beliefs, rituals, and so on, is particularly helpful.

Distinguishing between culture and culturally constituted elements allows us to differentiate between cultural meanings and identity on the one hand and structure, behaviors, and individual beliefs on the other. Structure, from this perspective, is reflective of (and to some extent derived from) culture, but it is independently measurable, and an important empirical question concerns the conditions under which the correspondence between culture and various culturally constituted elements is high and when it is not. We also can examine hypotheses about change and consider how culture, structure, and other phenomena do and do not shift in patterned ways.

Culture and Change

Cultures are commonly viewed as slow-changing entities, and there is some real merit in this position at times with respect to identities (Horowitz 2002). How, then, can the concept of culture help comparativists deal with issues of political change, especially relatively rapid developments such as the end of military rule in many Latin American states during the 1980s, the breakup of the Soviet empire, or the peaceful transition in South Africa?

Three points are worth making. First, cultural analyses are probably no better than any other partial theories, such as interest or institutional ones, available in comparative politics at explaining many change situations. But few of our theories are good at this at all. In addition, there are some change phenomena for which each is most powerful, and some aspects of change are surely not best explained in cultural terms. Second, interestingly, while it is not clear that cultural theories would have explained the fall of the Soviet empire very well (many other comparative political theories share this feature), a political cultural analysis is probably a good deal better at accounting for the ebb and flow of politics in the region in the early years of the transition than many of its rivals (Eckstein et al. 1998). Particularly in unstructured, changing settings, cultural interpretations and assumptions about the motivations of others can be especially important in accounting for political processes in which there are few or no institutionalized procedures to guide action. Third, few contemporary views see culture as a static, unchanging phenomenon marked by fixed beliefs and unalterable practices (Eckstein et al. 1998). Instead, the emphasis is on the interactive, constructed nature of culture, which suggests a capacity to modify beliefs and behaviors, including shifts and rearrangements in the salience of specific cultural understandings (Goode and Schneider 1994; Merelman 1991; Wedeen 2002).

Culture can play a significant role in change when change-oriented groups and their leaders find that change meets their core needs. They often can articulate culturally meaningful narratives to mobilize supporters, as occurred in South Africa during the transition from apartheid to democratic rule. Defining culturally legitimate possible alternatives both builds support and can challenge a regime. Brysk argues that this is particularly powerful when it involves reframing elements of identity in a way that mobilizes supporters, produces agenda change, and challenges the legitimacy and authority of existing policies and institutions (1995: 580–582).

Mechanisms Underlying Cultural Explanations

Asking “how culture works” raises two different questions: (1) how does the organization of any particular culture produce the specific effects attributed to it? and (2) why are appeals to cultural identity so powerful that people are willing to take high risks in its name? The first question is about the organization of culture, and the second is about its mobilizing power.

Theories giving culture a central explanatory role must specify how the effects attributed to culture come about (Berman 2001). It is not good enough to simply say, “They did it because they’re Germans.” While this statement implies that non-Germans (such as the Japanese or French) would have behaved differently, adding a clause to this effect doesn’t really enhance an explanation a great deal. Only when one starts to say why Germans are likely to behave in a certain way that is different from the way Japanese or French behave (in what is presumed to be an equivalent situation) do we begin to have an adequate explanation that pays attention to the content of culture and also says something about how it is learned and reinforced (Strauss 1992; D’Andrade 1992). Learning and reinforcement involve institutional contexts in which a person practices and then masters key behaviors, infusing them with emotional significance.¹⁹ Petersen’s study of ethnic violence calls on another mechanism – emotions – that narratives evoke at the micro level to explain uncentralized and uncoordinated macro-level behavior.

Social experiences within institutions such as schools, religious organizations, kin groups, and later in work and leisure settings all provide cultural messages about values and expectations that are selectively reinforced. It certainly is the case that the messages from different domains are not always fully consistent. Sometimes there is a difference in emphasis; at other times, there is an outright contradiction (e.g., peer groups and families don’t necessarily give adolescents the same messages). However, what is most important from a

¹⁹ Beatrice Whiting (1980) discusses the importance cross-culturally of the placement of individuals in particular contexts (e.g., girls take care of younger siblings more often than boys, and boys are more likely to take care of animals in all the cultures for which she has data). Her data also show that cultures vary in the settings they “make available” to individuals, and she distinguishes between behaviors seen as “mundane” and those domains that are “projective” and infused with great emotional significance.

cultural perspective are the beliefs, customs, rituals, behaviors, expectations, and motives that individuals internalize and that are widely shared among people in a culture even though they may also, at the same time, be highly contested. For example, Scott's (1985) study of a small rice-growing village in Malaysia shows how people can share meanings while at the same time competing over how specific elements are to be weighted and in what situations which cultural elements are most relevant. Culture is about what is believed in common and regularly reinforced; there is a reward for "getting it right" and a cost – which most people are willing to pay at times – for not doing so. Finally, it should be noted that cultural learning is not necessarily very conscious, occurring when individuals in institutional roles pass on culturally sanctioned beliefs and behaviors to others. Through these experiences, culture prepares people to make sense of – to interpret – the world and act "effectively" in it.

The power of culture – the ability to mobilize action in its name – requires explanation, for it is not always the case that people can or will exhibit solidarity around cultural identity just because a leader (or anyone else) asserts that there is an external threat. Cultural mobilization builds on fears and perceived threats consistent with internalized worldviews and regularly reinforced through high in-group interaction and emotional solidarity. Such worldviews are expressed in daily experiences as well as significant ceremonial and ritual events that effectively restate and renew support for a group's core values and the need for solidarity in the face of external foes (Kertzer 1988). In potentially threatening situations, the ability of a group to organize collective action, which can range from unified voting to political demonstrations and violent action, is tied to the plausibility of a specific worldview – although the view itself does not produce direct effects. Rather, these effects must be mediated through institutions (Laitin 1986, 1995). The resonance between the definition of a situation and group-based action is often not explicit, as Abner Cohen's (1969) analysis points out. Nonetheless, it is effective when group members act in unified ways in the face of perceived threats or to achieve collective goals.

Cultural Explanations Are "Just-So" Stories and Not Causal Accounts

A final criticism is that because the concept of culture itself is diffuse and cultural units are not clearly delimited, cultural explanations are not rigorous and not causal but simply produce "just-so" accounts. To begin to address this point, we should recognize that while not all cultural explanations aim to establish causality or employ its language, this is not always the case. As Fearon and Wendt (2003) point out in their essay on rationality and constructivism, there is a great variety in the epistemological preferences of constructivists, and there is similar variation among culturalists in terms of "whether knowledge claims about social life can be given any warrant other than the discursive power of the putative knower . . . and whether causal explanations are appropriate in social inquiry" (2003: 57). While some explanations, such as Petersen's (2002), are positivistic and causal in form, others, such as my own work on cultural expressions and

enactments, are less explicitly causal and emphasize the constitutive context that, in Fearon and Wendt's language, "seeks to establish conditions of possibility" more than causality as such (Ross 2007). Thus, while there is variation in cultural explanations, the kinds of questions selected for analysis and the predilections of particular researchers, not culture itself, determine the nature of the explanations that emerge.

CONCLUSION: LINKING CULTURE TO CHOICE AND INSTITUTIONS

Culture is a worldview that provides a shared account of action and its meaning and imparts social and political identities; it is manifest in a way of life transmitted (with changes and modifications) over time and is embodied in a community's institutions, values, and behavioral regularities. Politics occurs in a cultural context that links individual and collective identities, defines the boundaries between groups and organized actions within and between them, provides a framework for interpreting the actions and motives of others, and offers resources for political organization and mobilization. Cultural accounts of politics emphasize how, through shared intersubjective meanings, actors understand and act in their daily worlds. Beginning with context-dependent accounts – actors' worldviews – cultural analysis goes on to construct plausible interpretations of political life to explain individual and collective action.

I have argued for a "strong" view of culture and against the notion that it can be simply approached in terms of specific values that people in a community articulate. For one thing, the significance of the presence or absence of consensus on any single item is often unclear without an analysis of the context in which it is held. Culture as meaning and meaning-making is not at all incompatible with strong disagreement on particular attitudes, and I have argued that it is often those points of disagreement that are of real political significance and shed light on tough problems facing a society. No society is totally integrated or even in agreement on all important matters. As a result, a cultural analysis might utilize survey data to document the nature of divisions in a country, but it is good to go a good deal further, trying to make sense of why, how, and if specific values are important to people, the connections between value differences and political and personal identity, and the dynamics of bitterly contested political meanings and actions within a common cultural framework.

Cultural analysis can enhance our understanding of politics in a number of domains. McAdam et al. (this volume) describe important cultural contributions in the field of contentious politics and offer a model of how structural, interest, and cultural perspectives can complement each other in explaining politics. Seeing social movements as both carriers and makers of meaning, they suggest, enriches older, more developed structural and resource mobilization perspectives. Cultural analyses emphasize the framing of action, and increase our understanding of the definition of political opportunities and the repertoires of action that are found in different settings (Hafez 2007). Attending to narrative structuring and symbolic politics expands our capacity to explain collective

action in terms of changing preferences, changing identities, and changing responses to resources (Brysk 1995: 567).

Attention to culture would certainly address one of the most widely cited weaknesses of rational choice theory: its inattention to context-specific interests and cross-cultural differences in how interests are conceptualized and articulated (Levi, this volume). More broadly, political economy might be an area that would benefit from more explicit attention to cultural questions. For example, political economists have long documented differences in resource distribution across countries. While a certain amount of lip service is paid to “cultural differences” in these cases, a more profound analysis would inquire into cultural conceptions of social justice, linked fate, and perhaps the relationship between the individual and the collectivity that might help explain the low- (or high-) inequality cases. Similarly, there are probably important cultural factors involved in explaining cross-national differences in the locus of decision making and control over the economy. Where economic theory would emphasize efficiency, it may be that culture is much more salient in determining not only how a country comes down on such questions but also how it organizes the implementation of economic and political programs.

Cultural and rationalist accounts are not necessarily directly at odds, and there are a number of ways in which the two can complement each other (Fearon and Laitin 2000). Bates, de Figueiredo, and Weingast (1998) advocate examining expressive culture and emotions to reveal important political strategies key actors adopt. In making this argument, they recognize the role that historically significant narratives and fears can play in developing and selecting strategic options. In this way, cultural analysis can illuminate how interests and incentives are defined and framed for participants rather than having to assume that they are homogeneous across people and groups, as some do (also see Levi, this volume), and a more fine-grained analysis of interest dynamics that culturalists might provide could better ground game-theoretic analyses illuminating how signaling and bargaining work in specific contexts. For culturalists, this linkage could lead to a richer understanding of worldviews with regard to how they affect perceived choices and actors’ analysis of their strategic options. In addition, although culturalists can often identify key interests for specific groups or individuals, their analyses often are limited by their explanations of when and how actors pursue them. As a result, a more sophisticated cultural understanding of strategic choice-making would perhaps better account for when action takes place and when it does not. In sum, because actions do not speak for themselves, rationalist accounts might be more useful when actions are ascribed contextually defined meanings (Bates, de Figueiredo, and Weingast 1998: 620, 628), and cultural accounts of contestation among competing narratives might be better if they included a richer understanding of how actors understood strategic options.

Political institutions are another obvious candidate for more culturally oriented research, although such studies are certainly not totally absent. Many students of American legislatures, for example, have found culture particularly helpful in explaining their internal operation (Matthews 1962; Muir 1982). The

“folkways” of the U.S. Senate that Matthews identifies to explain its functioning in the 1950s are specific norms affecting any individual senator’s behavior but are also a system that cannot be understood simply in terms of its individual elements or the degree to which any senator thinks a particular norm is appropriate. Similarly, as Crozier (1964) demonstrated so effectively, culture can shape the behavior of both public and private bureaucracies, and his cultural model sharply contrasts with explanations derived from a more universal, rational-actor, bureaucratic routinization model.

Culturalists often proceed on the assumption that political institutions reflect a region’s culture while failing to examine this hypothesis very critically. While surely culture can shape institutional practices, there are other interesting possibilities as well that might connect the two: Institutional autonomy can produce an independent effect on culture and cultural expectations; institutional practices can alter cultural values and expectations; institutional arrangements can exacerbate or inhibit cultural differences and identities (e.g., federalism or regionalism or group-based resource allocation). When culture becomes institutionalized, it is more likely to remain significant over time, but we still need to better understand why and how this occurs in some contexts but not others (Berman 2001: 237–240). At the same time, cultural analysis could enhance our understanding of when specific institutional arrangements and practices are accepted and when they may prove to be problematic, as Eckstein and his cohorts have done in their analyses of post-Soviet democratization (Eckstein et al. 1998).

In conclusion, I have argued throughout this chapter that culture shapes a good deal of political behavior, defining what people fight and care about, how they pursue it, and what they achieve. Culture frames the rules that can guide political action even in the absence of strong institutions to enforce them, and it is crucial in defining the parties’ competing interests and divergent interpretations of what is at stake. Understanding actors’ interpretations and the shared narratives groups recount offers a powerful tool for examining political contestation that can provide comparativists with important insights into why politics takes the form that it does in specific contexts at particular points in time. The strong view of culture as shared meanings, meaning-making, and identity, while not rejecting the idea that there are important generalizations about politics that comparativists can and should draw, cautions us to be wary of those that pay little attention to contexts, perceptions, and meanings.

Researching the State

Joel S. Migdal

INTRODUCTION

States are wonderful subjects for comparative political analysis and, at the very same time, pose all sorts of thorny problems for any researcher studying comparative politics. They are good, meaty topics because a clear template exists for what a state is or, at least, what it should be, making diverse states eminently comparable. One might think of this template as the gold standard of states. Big or little, rich or poor, strong or weak, old or new, all states should conceivably measure up against this template, even as they deviate from it somewhat in this way or that. Later, I will discuss more fully what goes into this template, but one can get a rough idea by looking at two elements. Max Weber captured the essence of one of these succinctly: “The modern state,” he wrote, “is a compulsory association which organizes domination” (Gerth and Mills 1958: 82). Anthony Marx put his finger on a central second aspect, saying that the population not only serves the state but is *served by it*, and, in serving its citizens, the state generates allegiance by the population to it (Marx 2003). The state is both feared and loved.

But that gold standard, or template, has had difficulty being standard enough for the nearly 200 states that exist today. In particular, the rush of state creation after the two world wars, coming in the wake of collapsing empires, produced states for which scholars have been at pains to apply the standard template. Even when we look at states as unified, purposive organizations, whose structures are so remarkably similar across the globe, their actual *practices* vary so markedly as to seemingly make them almost impossible to compare, to measure against a single standard. Scholars have often felt that they are comparing apples and oranges. Researchers have been bemused by how far from the standard image states formed in the twentieth century have looked and acted. Weber’s language of domination and monopoly over the use of legitimate violence seems very remote from what many states have done in practice, as is the notion of populations being served by their states. Instead, the words used by scholars to

I would like to thank Ceren Belge for her research assistance on this chapter.

describe some of these states include “collapsed,” “failed,” and “criminalized.” Some states, willingly or unwillingly, have ceded tasks, such as security or tax collection – functions that are central to people’s image of what states actually are and what they do – to private corporations, militias, mercenaries, and local strongmen. State leaders, in cases such as Lebanon or Sudan, have made little pretense that their states actually exercise authority from border to border or that they speak for all the people – minimal criteria for “stateness” in the standard template (Nettl 1968).

Beyond even what some may consider the fringe cases of criminal or failed states, from the 1980s on, all states have faced increasing globalization, which in a number of ways has diminished both many states’ ability to dominate and the attachment of populations to them. Solingen’s chapter in this book highlights the large literature treating global and international sources of domestic politics as what she calls a “sine-qua-non” feature in the study of comparative politics. These international elements, too, have led in many cases to states that seem to lack stateness. Additionally, a burgeoning literature on “deterritorialization,” disappearing sovereignty (Sassen 2006), and new forms of governance has called into question the future of the state and the standards social scientists have used to study it.

In the coming pages, I will delve into the difficulties and promise of developing a comparative politics of the state in these circumstances. Some of the research that has appeared in the past decade points to some ways to surmount those difficulties. Work on the state in political science over the past decade, I will argue, has edged away

- from attachment to one paradigm – rationalist, structuralist, or culturalist – to a new eclecticism;
- from reliance on comparative statics to historically rich research, taking account of temporal sequencing and critical junctures;
- from the use of a single template of the state to approaches recognizing both the isomorphism of states and their great diversity;
- from the either/or of single-case studies or large-N research to studies of distinctive subsets of states, whose findings are in dialogue with, rather than merely validating, large-N hypotheses;
- from linear causal analysis to process-oriented, complex, multitiered research.

My central point is that the incorporation of these antinomies, indeterminacies, and eclectic approaches into research, even on a piecemeal basis, has already begun to move the comparative study of the state away from templates and areas of study that were limited in the number of actual states to which they applied, as well as the topics they could address, to a more promising frontier of historical, process-oriented research brimming with indeterminacy and unintended consequences. It follows Zuckerman’s lead in this volume in talking about the need for explanation through social mechanisms, studying stochastic, multilevel, and reciprocal phenomena.

The next section will briefly examine the difficulties that scholars have had in using standard paradigms to study the state, particularly rationalist and structuralist approaches, which, more often than not, assume an institutional coherence and purposiveness of the state. All in all, many scholars have reacted to the difficulty that new twentieth-century states have posed to standard tools for comparison by throwing up their hands, largely writing off many states as really not being states at all. The following section will look more closely at the two forces pulling at scholars: the use of the standard template and the growing diversity of actual states. In that section, I will examine the creation of states in the twentieth century, which have looked quite different from the standard template scholars have used and pose problems for those seeking a comparative politics of the state. The latter portion of the chapter will report on two clusters of new comparative research, political trajectories and integrated comparative analysis, that have begun to cut through the Gordian knot of creating a broadly applicable and compelling comparative approach. I will note that even these approaches tend to lean much more heavily on structuralism and rationalism, neglecting cultural factors. I will then point to some emerging research that has attempted to nurture a culturalist perspective.

THE DIFFICULTY IN COMPARING STATES

The notion of states as unified and purposive or of their institutions as integrated, coherent units has run up against actual practices of numerous twenty-first-century examples. Whole states and individual bureaus have frequently been internally divided, with different parts allying with a variety of domestic and foreign groups and organizations. Fragments of states and *their* allies have frequently been pitted against other fragments within the state (and even within single state institutions) and *their* allies. The crazy-quilt alliances and coalitions in which those in the state organization engage have frequently undone the role of institutions within the overall logic of the state. And the ties that have united the alliances of state and society and set those against contending alliances have often subverted the notion of unified institutions with recognizable missions.

The apparent lack of sufficient stateness of many juridically recognized states has created a problem for those scholars attempting to universalize and standardize states, particularly those using structuralist and rationalist approaches, stressing a singular template of states and insisting on their comparability. They have run up against the problem of the single standard not being standard enough. The radical diversity of states has led some scholars to question whether some are really part of the genus of states at all.

Simpson has written, for example, of *outlaw* states, with their “estrangement from the community of nations and demonisation by that community”: “These states are mad, bad or dangerous, or all three.” The dangerous ones, he claims, suffer from “some internal malfunction or propensity to disorder” (Simpson 2004: xi). Other scholars have labeled those states that seem to be far from the imagined European standard as “quasi”-states or “proto”-states or “collapsed”

states or “fragile” states or (a popular label these days) “failed” states (e.g., Chomsky 2006; Jackson 1990; Rothberg 2003, 2004; Zartman 1995).

A more broadly based comparative politics of the state demands overcoming some of the limitations posed by the existing template of the normative state, what I have called the state’s “image” (Migdal 2001; Migdal and Schlichte 2005). It would be a mistake, I think, to simply discard altogether this image of the state as an analytical tool. After all, it provides a picture of what states are supposed to do that is widely accepted, not only among scholars but by laypeople the world over. And it takes account, too, of the remarkable similarity in form of contemporary states everywhere. I look into this uniform image of the state in the next section of the chapter. But this normative image must be taken into account in the context of the actual maddening diversity of states. These states differ from each other in every imaginable way. From their ability to do what their leaders say they will do to their relationship with their populations, states come in all stripes. I use the term “practices of the state” to denote what states actually do, whether they fit the standard normative image or not, thus capturing the vast differences among them. It is, perhaps, the distinctiveness of states’ practices, the “questions about experience for which answers are wanted,” in the words of Harry Eckstein (Eckstein 1992: 130), that has pushed scholars toward what Mark Lichbach (drawing on Eckstein) in this volume labels the “messy center.” Indeed, the distance of many states from the standard makes general comparison a risky business. I will take up state practices following the discussion of the state’s image.

For the moment, it is important to note that any definition of the state that can be used as the cornerstone of a broad comparative politics of states, which does not simply relegate some states to existing beyond the academic pale, must capture the duality of the singular ideal-typical mold of the state, its image, as well as the diversity that one sees day in and day out in its practices. What follows is one definition that tries to capture this two-sided peculiarity of the state, recognizing states’ isomorphism, the near-universality of their image, while capturing the diversity of what real states actually practice in their administration, their ties to their populations, and their relations with other states and transnational actors:

The state is a field of power marked by the use and threat of violence in attempting to control people’s behavior and shaped by 1) *the image of a coherent, controlling organization in a territory, which is an outgrowth and representation of the people bounded by that territory*, and 2) *the actual practices of that organization’s multiple parts*. (Migdal 2001: 15–16)

IMAGE AND PRACTICES: THE TENSION OF A SINGULAR TEMPLATE AND MULTIPLE DYNAMICS

The Image of the State: The Two Faces of Domination and Allegiance

The widely held image of the twenty-first-century state supposedly derived from the actual history and reality of the first and most formidable states, those in Northwestern Europe, starting half a millennium ago. Even if that European

“history” turns out to be more myth than fact, the image itself has taken hold among scholars and laypeople alike as a mirror against which all states can be held up. The striking structural similarity of states, with their foreign ministries and treasuries, their legislatures and courts, helps constitute the singular image. It has become embedded in people’s imaginations, not only among populations living under the rule of highly effective centralized states, but even among those suffering the unruly, deadly conditions where no organization, state or otherwise, has managed to establish control. Whatever its derivation, then, this image of the state has become widely shared in different cultures and continents; what a state is, or should be, at least in the mind’s eye, has become remarkably similar across the globe, at least since the middle of the twentieth century and perhaps even earlier. For social scientists, the image provides a fairly well-agreed-upon standard for comparative analysis.

The image portrays a Janus-faced state. The first face is that of a coherent, competent body making and executing the prime rules governing people’s lives – state law, the central common institution of all states – across a broad, precisely bounded territory. It threatens dire consequences for any who flout its authority, whether they come from inside or outside its borders. In this picture, the state is complex, yet singular – integrated, coherent, autonomous, and operating on all cylinders (Huntington 1968). As a well-greased machine, it exercises extensive social control. Its cohesiveness and seeming replicability from place to place and from era to era make it an attractive subject for study, especially for those using a rational choice or structural paradigm, which home in on the purposiveness and complex, coherent institutional makeup of the state.

The second face is one that puts much less emphasis on the hierarchical and dominating qualities of the state. This interpretation posits it as intimately, even organically, tied to its population. Here, it is the voice of the people in its bounded territory – the citizens of the state – and it draws succor from these citizens. If the first visage connotes social control, the second conjures up warmer tones, such as representation, loyalty, and mobilization. If the first suggests the cognitive and the rational, the second intimates the affective and the emotional. Here, the state does not so much stand over the people as it stems *from* them or *embodies* them. The population is not simply an aggregate of diverse, rational individuals but a collective that transcends those individuals and that gives birth to, and then loyally engages and stands behind, the state. The state, then, is the representation of the general collective will of the people, even as it controls them. It derives from the people’s unity and then itself polices that unity, deterring its subjects from stepping out of bounds and punishing them when they do.

This notion of a transcendental unity of the people – the very idea of the “nation” – is very important in the image of the state. Unity overrides the real, inevitable divisions among the population: different passions, interests, economic and social situations, regional locations, backgrounds, affiliations, bloodlines, and more. The nation’s social unity gives the state the people’s collective consent, or legitimacy, to govern. This term, “legitimacy,” with all its

soft edges, transcending aggregated individual preferences, is one that political scientists seem incapable of escaping. While the image of the state as representing the nation is fairly uniform across time, in fact each nation lends its particular state a distinctive hue or personality. This distinctiveness within a unitary image makes the state – especially this affective side of the state – a particularly attractive research topic for those using a cultural paradigm.

The prevalent image of the state, then, is paradoxical. On one side is the hard, social, controlling state, captured in Weber's characterization of it as a "relation of men dominating men" through its claim on "the *monopoly of the legitimate use of physical force* within a given territory" (Gerth and Mills 1958: 78). And, on the other side, is the softer representational state, growing out of the social solidarity and general will expressed by Durkheim and Rousseau. In Rousseau's words, "The general will alone can direct the State according to the object for which it was instituted, i.e. the common good" (Rousseau 1950: 395).

Of course, neither those who stressed domination, such as Weber, nor those pointing to representation, such as Durkheim, saw their defining characteristic as exclusive. Weber, after all, sneaked the word "legitimate" into his talk of monopoly and physical force. Likewise, while Durkheim saw the state not as autonomous but as inseparably bonded to society, he still emphasized the agency of the state (or its will and purposiveness, if you like): It is the state that can "take account of the general needs of life lived in common and prevent these from being subordinated to individual interests" (Durkheim 1986: 49). To Durkheim's mind, the essence of the state draws from its location at the central point where all will come together. It is the point of intelligence, bringing together "those sentiments, ideals, beliefs that society has worked out collectively and with time, and that are strewn in the consciousness of each one" (Durkheim 1986: 54). Nonetheless, Durkheim, Rousseau, and others of their ilk have stressed that it is the nation that collectively affords the state, in the words of Harold Laski, a "unity of allegiance," even as the state guards the collective national interests of the members of the nation (Laski 1917: 11). The state, in this softer image, draws strength, in Laski's words, from the fact that "man is a community-building animal," accounting "for the unique claims [the state] has made upon the loyalties of men" (Laski 1919: 19, 21).

Anthony Marx pointed to the nation as the underlying connection between the hard and soft parts of the image, the two faces, of the state. He defined the nation as "the popular loyalty of a population held together in being obliged to serve and be served by the state. By encouraging allegiance to the nation, states enhance their claimed monopoly of legitimacy. Accordingly, state elites and others have been inspired by the goal of the nation-state, a coincidence of institutional rule and allegiance to it that would diminish internal conflict" (Marx 1998: 4).

The prevalent image of the state, then, is incongruously all about exercising authority to tame wildly *different* wills and drawing on loyalty that comes from a *general* will. Authority involves demanding obedience from the population and loyalty means gaining support, often voluntary support, from that population. The state is at once dominator and supplicant.

Both exercising authority and gaining loyalty have posed similar challenges to every state, especially as each took form: How would it govern an extremely heterogeneous population made up of multiple – often openly combative – religious, ethnic, tribal, linguistic, cultural, economic, and national groups and of individuals with vastly different preferences, sentiments, and needs? And, second, how could it gain that all-important unity of allegiance of the nation so that it could win over – and mobilize – the population to pay for the maintenance of the state and defend its borders?

The similarity in challenges from state to state and the standard image of the state, in general, have proven very useful to political scientists using rationalist and structuralist paradigms. Indeed, as Rudolph and Jacobsen have made clear, “much of the literature in political science and the social sciences more generally treats ‘the state’ as an abstraction by universalizing and standardizing what the state is, does, and means. States are discussed as if they were the same through time, space, and circumstance” (Rudolph and Jacobsen 2006: vii). In this way, social scientists aggregate and compare states; by making states (and nations) integrated, coherent units that are amenable to comparison, scholars (as well as journalists and others) frequently anthropomorphize their units of analysis, imputing desires, goals, and a will to states and their populations.

The Practices of the State: The Growing Diversity of States in the Twentieth Century

As the tone of the preceding quote from Rudolph and Jacobsen hints, a current of skepticism, even discontent, about this universal image runs through the social sciences as well. After all is said about the comparability of states – their single mold, or image – actual states show amazing diversity, straining the idea of a unitary image and making comparison of them a messy business. They vary tremendously in the way they interact with the societies they purport to govern and represent – and in the degree to which they actually do govern and represent. Although state institutions – their executives, bureaus, legislatures, judiciaries, schools, prisons, armies, and the like – may, at first blush, look astonishingly similar or, at the very least, fall into one of several identifiable types, making them eminently comparable, on closer examination those institutions act in myriad different ways. Comparison may privilege form over function; states’ shells may resemble one another, but the all-important innards are strikingly dissimilar. Scholars studying the distinct makeup of individual states have often come from a culturalist perspective, emphasizing the particularity of the functioning of individuals and institutions rather than their commonality.

In tracing the construction of the modern international legal order, with its understanding of sovereignty, over several centuries, Simpson, in effect, presents the development of the singular image of the state, that gold standard upon which political scientists have relied. The reification of that image explains, in good part, how scholars have reacted to the actual baffling diversity of states that developed in the twentieth century. As Simpson notes, along with the

development of the state's image has come the distinction made by jurists and scholars between *true* states and some sort of incomparable mutants. The international legal order as far back as the early nineteenth century distinguished between European states and a heterogeneous group of others, including "uncivilized" states. Indeed, in the epigram of chapter 8 of his book, Simpson quotes Hegel's *Philosophy of Right*: "The civilized nation is conscious that the rights of the barbarians are unequal to its own and treats their autonomy as only a formality" (Simpson 2004: 227). While the tendency to wall off these "uncivilized" states from the category of a *true* state waxed and waned over the twentieth century, Simpson is firm in claiming that a specific form of liberal antipluralism took hold in that century; such intolerance to certain kinds of states – collapsed, proto, outlaw – as real states seems only to have deepened in the twenty-first century. Labeling them as failed states or quasi-states or anything of that sort is a reflection of such antipluralism, leaving these twentieth-century variants out of what scholars considered *real* states. As one article notes, these words are *ex-negativo* characterizations and "do not tell us much about how politics work" (Isachenko and Schlichte 2007: 4). Such exclusion may simplify experience enough to allow the use of one paradigm or another cleanly, but it does so at the risk of ignoring how many, if not most, of the juridically recognized states in today's world actually look and work.

What occurred in the process of state formation in the twentieth century to cause such antipluralism to take hold? What led thinkers to place large numbers of states outside the categorical boundaries of statehood? At the simplest level, the twentieth century witnessed a tremendous growth in the sheer number of states. And most of the new state formation came in different geographical regions – outside the core area of the first states, Europe. Beyond numbers and place, though, the global circumstances producing new states in the past century strained the image of the state and models derived from it that scholars employed in analyzing states, even as those new states were ostensibly born out of that image. What were the world-historical conditions of the twentieth century that so deeply affected newly emerging states' structures and capabilities and that made the old gold standard, the singular image of the state, a diminished analytical tool?

In fact, there was no single set of world-historical conditions affecting state formation in the past century; three waves of twentieth-century states faced different world conditions at their inception, which helped shape their political trajectories and the process of state formation. Each of the three waves (perhaps "tsunami" would be a better metaphor) was set off by the collapse of major empires that had ruled vast expanses of the world's land surface and multiple, diverse human communities. These empires died for a large number of reasons, but among them was the pressure put on by nationalist movements pushing for self-determination, their *own* states, by multiple groups living under the empires' broad umbrellas.

World War I and the collapse of the Russian, Austro-Hungarian, and Ottoman empires reshuffled political rule in Central and Eastern Europe, the Middle East, and what became Soviet Asia. A second wave created an unprecedented number of

states in Africa and Asia as the European colonial empires expired. And, finally, with the collapse of the last great empire, the Soviet Union, in the final decade of the twentieth century, along with the breakup of Yugoslavia and Ethiopia, a smaller wave of new states appeared. Each of the three waves produced a distinctive set of conditions that helped shape state formation, leading to increasing diversity in the world family of states. I will look at the first two of these waves in the twentieth century, which provide a telling contrast of variations in the conditions of state formation in different historical circumstances.

The first round of twentieth-century state formation ran headlong into the nationalist problem. Different ethnic and religious groups, which had coexisted, even if sometimes uneasily, in the big tent of nonnationalist empires, now faced off in states whose very names often included one of the groups and, rhetorically at the very least, excluded others. Romania, to take just one example, won its independence even before the beginning of the century, from an already shrinking Ottoman Empire, after a series of initiatives in the 1860s and 1870s. In its various incarnations since then as an independent state, it included a majority of people who had come to think of themselves ethnically as Romanians, but also a sizable minority of Jews, Gypsies, Hungarians (Magyars), Germans, Ukrainians, and others. The very name and construction of the state privileged ethnic Romanians and disadvantaged these other minorities. State formation in Romania and other parts of Eastern Europe and the Balkans, as a result, took place in an environment in which fractious ethnic and religious groups confronted one another, and, what may have ultimately been more important, some of these groups had fellow group members on the other side of the newly formed state boundaries. The term "nation-state," implying an organic tie between a homogeneous population and the state that governed and represented it, turned out to be more of a cruel joke than an accurate descriptor. Rousseau's general will, Durkheim's social solidarity, and Laski's unity of allegiance did not hold in Romania or most of the other states that came out of the crumbling empires.

State leaders in this new wave of states, especially after World War I, were keenly aware of the threats posed from outside their borders, particularly when those doing the threatening had allies inside those borders. These political leaders had strong incentives to establish institutions to gain material and nonmaterial support from at least the group that shared its name with the state, the titular group, and possibly some others as well. That support could spell the difference between survival and destruction of their fragile states. In short, in the wake of the Great War, states faced international and regional environments in which outside threats, invasion, conquest, border changes, and even state disappearance were real possibilities. The history of the modern state system from its inception in the sixteenth and seventeenth centuries had been replete with states devouring each other, and this period, for all its talk of outlawing war, did not seem much different: Leaders in the Balkans, Eastern Europe, Russia, and Turkey understood how precarious their new borders and the state itself were. And, indeed, within the next generation, many of the new states did face direct invasion and either elimination or subjugation.

Survival depended on state leaders' establishing domination, collecting taxes, building loyalty among core groups, and, not least, mobilizing an effective army. Despite all the high-sounding idealism of the Paris Peace Conference, the Versailles Treaty, and Woodrow Wilson's insistence on self-determination, leaders of new states, such as those in the Balkans, understood that they faced existential, irredentist threats from neighbors, often allied with hostile ethnic minorities and/or major powers. The Soviet Union and Turkey, which arose out of the ashes of the Russian and Ottoman Empires, were the first to face wars, both international and civil/international, that threatened their revolutionary leaders' barely won hold on power. Later, practically all the new states of the time had to fend off hostile neighboring states while disciplining their own populations – or the parts of them that leaders felt they could trust – and garnering from citizens badly needed revenues and bodies to defend the borders.

Those challenges pushed leaders to make the new state, with its particular (often peculiar) boundaries, seem meaningful to a majority, or at least a plurality, of the population. It sometimes meant, too, that minority populations – identified by state leaders as dangerous populations – were used as foils or even scapegoats, as a means of building allegiance in the titular group. The details differed, depending on local conditions, but the political trajectory created a common set of conditions and responses for practically all the new states of the time. Leaders used the “dangerous” populations inside the state's borders as evidence that a threat was palpable, close at hand, residing even among one's own neighbors. The idea of a nation-state and the slogan of “self-determination,” so popular in the rhetoric of the day, rang hollow in actual practice. In reality, political leaders found themselves with large religious and ethnic minorities, which most often did not accept the new state as truly theirs and frequently were used by these leaders as negative symbols for rallying the dominant ethnic group.

If these were not states with single nations, many still were able to build relatively effective state institutions that could extract resources from the dominant, if not exclusive, national group and even, at times, from other groups; gain loyalty and shared identity from the dominant group, even as the leaders themselves constituted a small clique that monopolized privileges and key state positions for themselves; and, finally, repress the restive minorities. The loyalty and identity they tried to evoke from the members of the titular group involved convincing them that the state's (often rather arbitrarily created) boundaries were sacred to the group and worth defending even at great personal sacrifice, including the ultimate sacrifice of one's life or the lives of one's sons.

In short, states that came out of the political traumas of World War I followed some of the same routes in state formation found among earlier Western and Central European states. They built relatively effective police and military forces and bureaus to collect taxes; they looked after the food supply and issues of public order (Tilly 1975). But world conditions led state leaders to very different relations with their overall populations. Nationalism had already made significant inroads by the time World War I broke out, at least in Eastern and

Southeastern Europe, if not the Middle East. The opportunity to create “national states” (as Tilly’s book calls them) in these regions, with the large majority of people in the state’s territory identifying itself with the new state, was simply not present. A key aspect of the standard image of the state, involving such terms as “general will,” “legitimacy,” “social solidarity,” and “unity of allegiance,” simply did not apply in these new states.

A very different set of historical conditions existed for the leaders of the new states created after World War II. It was then that a great number of states, created in a short time span, were formed, starting in 1947 with the independence of India and concluding barely two decades later as decolonization drew to a close in central Africa. All in all, decolonization just about tripled the number of states in the world. State leaders in this wave, too, had witnessed the precariousness of other state boundaries, both during the war and in its immediate aftermath. But postwar circumstances, most leaders quickly learned, were quite different.

Now, leaders’ motivation to face the challenges of building institutions to engage the population, establish domination, mobilize resources, and create loyalty quickly melted away. Why? Certainly, the costs of building such institutions were enormous; projecting authority over a large territorial expanse and a heterogeneous population was no mean feat. “All leaders face costs when trying to expand their writ of authority” (Herbst 2000: 23). But that was as true for Balkan state leaders after World War I as it was for African leaders after World War II. And, for many of the new states born in the wake of World War II and the subsequent collapse of the colonial empires, their populations were every bit as heterogeneous as those that emerged after the First World War. “Dangerous” populations were readily at hand and were no less an incentive to mobilize the key ethnic or tribal group (e.g., the titular group) than was the case after World War I. Why, then, did state formation among the set of states formed in the disintegration and collapse of the Austro-Hungarian, Russian, and Ottoman empires differ so markedly from that of the set of states coming out of post-World War II decolonization? Why were so many of the earlier ones able to mobilize at least the titular population effectively, while many of the latter failed miserably even at that task? Why were most of the earlier ones able to establish control throughout most of their territory, while the latter often failed to impose such control over large chunks of their population?

Herbst suggests that the low population densities in Africa – relatively unevenly and sparsely peopled vast hinterlands – led leaders to be urban focused, leaving huge swaths of territory basically unpenetrated by state institutions (Herbst 2000). That may be the case, but geography was not the only issue; historical circumstances played a crucial role in keeping these states from mobilizing people and cash as well. In the postcolonial world, the leaders’ old fears of their borders being overrun, putting the entire state at risk, faded. The Cold War, in contrast to the crumbling balance of power of the interwar period, established an eerie stability for even the most hapless states. Again referring to Africa, Herbst noted, “Given the lack of security imperative, which forced European state-builders to place

assets at the frontier at a loss, African statemakers were able, within certain constraints and given historical circumstances, to make much more nuanced calculations about costs and benefits of broadcasting power" (Herbst 2000: 23).

After 1947, states survived, whether they could defend their borders or not. (It is worth noting that academic literature in political science and international relations was remarkably uninterested in issues of borders and boundaries during the nearly half-century after World War II.¹ Only in the 1990s, following the end of the Cold War, the collapse of the USSR and other states, and, by no means least of all, Iraq's invasion of Kuwait did a new spate of articles and books return to the question of borders – a renewal of interest in geopolitical analysis, which had marked the pre-World War II era.) In any event, leaders no longer had the same strong compulsion to pay sky-high costs – both materially and in terms of political capital – in order to secure their boundaries. They did not feel an overwhelming compulsion to establish the kind of relationship with at least a substantial portion of their population that would enable them to mobilize sufficient human and material resources to keep predators at bay (Herbst 2000). State leaders in this era were much more likely to rely on small slices of the population to maintain their positions. Strong ties to a tiny portion of the new state's population in places such as Iraq and Syria, even as a mass party, the Ba'ath, purportedly mobilized citizens broadly, was the name of the game. Iraq and Syria, which had received nominal independence from Britain and France, respectively, a bit earlier than others in this wave of decolonization, achieved real autonomy from the colonial powers only after World War II. In Iraq, Saddam Hussein, who ruled from 1979 until 2003, drew largely on a narrow base from the town of Tikrit; and in Syria, Hafez al-Asad, who governed with an iron hand as well, from 1970 to 2000, depended on select families among the Alawi sect, a distinct minority, often considered heretical by the dominant Muslim Sunnis in the country.

The Cold War had another effect on state formation in this wave of new states beyond reducing threats to their borders: Postcolonial states did not face the same imperatives for engaging large swathes of their populations to raise cash, as did states formed in earlier periods, for waging war or even for simply maintaining the state in lavish style. Resources came, instead, from outside the country's borders. Indeed, the second half of the twentieth century saw an unprecedented transfer of resources across borders, a good portion of which ended up in the coffers of newly formed state organizations and in the hands of the narrow clique of state leaders. Foreign aid, direct foreign investment, and international loans poured into new states in Africa and Asia, as well as into older states there and in Latin America. From 1950 until the end of the Cold War, foreign aid for economic development totaled well over half a trillion dollars (more than a trillion if 1989 dollars are used), and net private investment infused almost the same amount (Lumsdaine 1993). After the huge rise of oil prices in

¹ An exception is Touval (1972).

the 1970s, petrodollars recycled through key international banks and ended up, mostly as loans, in the treasuries of new states. These massive transfers of cash meant that states could bypass the painful process of mobilizing their own populations to produce badly needed revenue. Why take the painful route of squeezing the population when formidable sums were so readily available internationally?

But this decision to take the easy road had unanticipated negative consequences. Generating revenue from a state's population, it turns out, has all sorts of unexpected ramifications for the relationship between states and societies. Among these is growth in the state's capabilities. Indeed, the ability to raise taxes can be seen as a proxy for assessing the state's capacity. After all, squeezing taxes out of millions of citizens is no mean feat. Building state mechanisms to accomplish that goal brings not only revenues but also an increased ability of the state to monitor its citizenry, both in terms of control and learning about people's needs, wants, and capabilities – that is, gaining valuable feedback. Taxation, in sum, is an excellent lens into state–society relations, if for no other reason than that state officials must learn a tremendous amount about the state's subjects if they are to succeed in collecting revenues from them. Unfortunately, studies of the state through research on taxation were few and far between during the Cold War era (Ardant 1975; Braun 1975; Levi 1988; Organski and Kugler 1980; Webber and Wildavsky 1986).

As an aside, it is gratifying to note that in the post–Cold War era the issue of taxation has become a serious subject of study among comparativists. A spate of recent important studies has emphasized the importance of taxation in assessing the relationship of populations to the states that govern them (Lieberman 2003b; Remick 2004; Slater 2005; Smith 2007b; Steinmo 1993). To my mind, the consideration of taxes in understanding the complexity of state–society relations is essential. As Slater notes, the challenges for states in collecting taxes are immense: A huge organization to perform the task must be built, and the revenues usually must come from the most powerful members of society, elites with substantial wealth (Slater 2005).² Or, as Elizabeth Remick puts it succinctly, when the state tax apparatus is successful, it reaches downward (Remick 2004: 24)

Additionally, collecting taxes induces states to move away from relating to their populations through either benign neglect or coercion exclusively. Successful tax collection depends in some measure on voluntary compliance, in generating support among the population for the state and its purposes, in identifying with the state. When states can survive on transfers from abroad – or, for that matter, rents (such as oil revenues), which also do not demand

² Indeed, taxation is only one of several topics involving critical dimensions or functions of states that are largely ignored in the literature on states. Another notable one is state construction; after all, one of the primary ways, if not the number one way, that states spend their money is on building everything from roads and bridges to courthouses and schools. The politics surrounding all this construction offer a rich field for analysis.

complex tools of revenue extraction from millions of people – there is not much motivation for political leaders to recruit groups beyond their core supporters to finance the state or to share in state power in exchange for the support they lend. Like the threat of war, the need to generate revenue from the population induces a drastic transformation in the relationship of rulers to ruled. The absence of the threat of war and the availability of alternative sources of revenue created the opposite effect in most postcolonial states from what Tilly described for Europe: Many postcolonial states were controlled by a ruling group that, while professing the glory of the nation and putting itself forward as the representation of that nation, remained insular and removed from the mass of the state's citizens, even from a good portion of the titular population. Without the need to depend on social groups for financial support, state officials retained high barriers to guard against the entry of others into their exclusive club.

The decline in threats to states' borders, combined with the inflow of capital, helped shape state organizations in the period after World War II. Some states, such as those in Lebanon and Sudan in recent years, have not had a semblance of control over violence inside their borders and have even promoted, or tolerated, nonstate militias engaging in violence against portions of the population. Others, such as in Burma, have established tight control in a limited part of the state's territory, usually around the capital city, but have been hapless in the hinterlands (Callahan 2003). Many postcolonial states that gained their independence in the 1940s, 1950s, and 1960s had little real ability to defend their borders in case of serious attack, as was evident in Kuwait in 1990, or to mobilize their population as soldiers or taxpayers. Yet, they created a state system that, mostly anachronistically, still portrayed each state as the premier rule-maker for the people within its territory, as a sovereign presence largely unaccountable to outside rules or groups. That is, they were incorporated into the global image of the state, based on that old European myth of what states are and should be. The notion of sovereignty, in Stephen Krasner's words, truly became "organized hypocrisy" (Krasner 1999).

The world-historical conditions of the twentieth century helped produce new states that fell far from ordinary people's expectations of what states should be, causing deep disillusionment and cynicism among many populations outside Europe. These conditions also affected scholarly discourse, as new states appeared so far from the singular image upon which scholars had come to rely. The waves of state formation after each of the world wars, especially after World War II, with their prototypes that seemed so distant from the standard image, led to a funneling of the comparative study of the state; scholars narrowed the universe of what they took to be *real* states at the same time that the actual numbers of states and their diversity were mushrooming. True, there had been an initial burst of optimism among researchers and policymakers alike that newly independent states would undergo "political development" and, in true Cinderella fashion, emerge from the pumpkin as European-like in form and function, coming close enough to the gold standard of states to allow useful

comparison. But as the new postcolonial states, along with some long-standing ones, stumbled into multiple coups d'état, never-ending civil wars, criminalization of state functions, and prolonged economic stagnation or even decline, many scholars and policymakers found their own approaches and theories of universal development and modernization sorely tested.

Scholars' perception of the differentness of these states – especially manifest in regime instability, lack of standard capacities, and their relationship to their populations (falling far from the nation-state ideal) – only deepened as other late-twentieth-century factors battered their shaky sovereignty (Camilleri and Falk 1992; Keller and Rothchild 1996; King 1991; Lyons and Mastanduno 1995 – Solingen in this volume has many more relevant citations). Globalization, with its exponential increase in the flow of capital and rapid growth of trade, strengthened international market ties, making their political borders seem more like sieves than barriers (Jameson and Miyoshi 1998; Mittelman 1996; Nandi and Shahidullah 1998; Perlas 1999). Noneconomic links spearheaded by the Internet and other advances in communication also connected peoples across national boundaries, often bypassing central state authorities altogether (Duchacek, Latouche, and Stevenson 1988). The end of the twentieth century and the beginning of the twenty-first battered all states in many ways, but some were able to maintain, even increase, their capacities, while others floundered (Waller and Linklater 2003). Many of the new, diverse array of twentieth-century states found their chronic problems of an already shaky relationship to their populations and their leaky borders exacerbated by the rapid globalization; many seemed to be ruling but not governing, as one recent title puts it (Cook 2007). And this was true not only of the states created after the world wars but also of those that came out of the collapse of the Soviet empire near the end of the twentieth century (O'Dwyer 2006).

Political scientists began to lose their enthusiasm for comparative approaches that included these faltering states. As new states took unfamiliar, often puzzling, paths of political change, the application of new scientific methods – behavioralist, rationalist, and institutionalist – to them became problematic. Structured comparison of the universe of states became very difficult, compounded by the paucity, and sometimes purposeful distortion, of standardized data. By placing unruly states outside the pale, scholars could hang on to their gold standard, their singular image of the state, as well as their scientific theories and methods.

NEW APPROACHES TO THE COMPARATIVE STUDY OF STATES

Only in the past few years have signs emerged of reversing this notion that many, if not most, states are really not states at all. Less wedded than American political scientists to models based on the hard sciences and mathematical economics, several European political scientists were among the first to offer new ways to undertake the comparative study of states. A number of them came from African studies, building approaches that could be applied to the broad expanse of states

(Bayart, Ellis, and Hibou 1999; Hibou 2004; Schlichte 2005). But there has been a growing groundswell in recent years in the United States as well, seeking new ways to study the wider universe of states comparatively.

Political scientists developing this new scholarship on the state have felt that researchers need to do more than throw up their hands at the idea of incorporating postcolonial states into comparative approaches and theories.³ They have shown little patience for formal models or for modes of comparison that insist on strict boundaries between rational, cultural, and structural approaches. Their work has moved away from overreliance on an “image”-based understanding of the state, derived from the European experience, and has incorporated, as well, the disparate practices of diverse states, even ones that do not hew closely to the European-state image. Stressing the importance of each state’s practices, the new research has affirmed the importance of the vast differences in states’ practices in engaging their populations (or not), which may be lost by forcing states into a constricting image. Indeed, these differences in engaging populations are salient even among states that cannot be thought of as collapsed or outlaw. But while not denying the vast disparities in state practices – indeed, even celebrating those differences – these scholars have also rejected approaches, cultural or otherwise, that treat states exclusively in an idiographic fashion, making each entirely unique. They have sought broader explanations and interpretations for state capacity, structure, behavior, and more. At the same time, they have broadened the understanding in comparative politics of what a state is, moving away from previous binaries that dealt largely with non-European states in terms of what they are not (or are not *yet*), namely, proper European-like states.

Several clusters of this new scholarship have developed in recent years, which hold the promise of enabling a broadly gauged comparative politics in studying the full gamut of widely recognized juridical states. Practically all the studies in these clusters have grappled with the tension between a standard image of the state and the very diverse practices of actual states. Some works have cut across the rational/structural/cultural divides in new and interesting ways, although, as I will mention later, the cultural approach still seems generally to get short shrift, while the structuralist, or institutional, approach appears to maintain a privileged position theoretically.

The research in these clusters has aimed to combine specialized country or area knowledge (which usually is focused on the different practices of diverse states) with more general theories of state formation and behavior. This eminently reasonable combination has made the sound and fury of the long-running debate between area specialists and general theorists (particularly, of late, those using a rational choice approach) seem somehow antiquated (Bates 1996; Johnson 2001; Hanson forthcoming). The works in these clusters have also looked closely at the engagement between transnational, even global, forces, on one side, and local, often idiographic, factors, on the other, (e.g., Whitehead 2001).

³ See the chapter in this volume by Isabella Mares on how postcolonial states are now being drawn into the comparative study of welfare states.

I will look at two clusters of research, one focusing on “political trajectories” (using Bayart’s term) and the other on “integrated comparative analysis” (borrowing Smith’s phrase) (Bayart 1991: 51–71; Smith 2007b). My own partiality is for the political trajectories approach (Kimmerling and Migdal 2003), but I believe both clusters show considerable promise in developing a truly comparative politics of states.

Both approaches have grown out of established research traditions in political science. Political trajectories research stems from what has been labeled “comparative historical analysis,” an approach that has gained popularity in recent years (Mahoney and Rueschemeyer 2003). While the broad category of comparative historical analysis casts a wide net, using path dependence and a variety of other historical approaches by social scientists, political trajectories research more narrowly refers to the intertwining of large-scale regional or global forces, on the one hand, with distinctive local ones, on the other. It aims at explaining the combination of the general with the particular over an expanse of time rather than analyzing it in a frozen moment. Comparisons frequently single out a subset of states in space or time (e.g., ones in a particular geographic region or ones formed in similar world-historical conditions), which have, as a result, experienced similar changes or pressures. And this approach follows how those forces have affected the individual states in different ways as they have encountered a variety of particular circumstances.

Integrated comparative analysis developed as part of a larger research tradition combining big-N quantitative analysis, using aggregated data, with specific case studies. The need to combine qualitative and quantitative analysis has been a continuing refrain in political science, most famously expressed in *Designing Social Inquiry* (King, Keohane, and Verba 1994a), as well as some of the fine work by Charles Ragin (Ragin 1987, 1994a). Much of the work in this vein has used case studies to draw out and substantiate broader propositions coming from the quantitative analysis. Lichbach, in his opening chapter, has suggested a somewhat inverse way of relating the two kinds of analysis through “stylized facts.” Integrated comparative analysis does not simply use country studies as illustrative of the range of cases in the larger N, nor as validating the general propositions generated by the aggregate study, nor as vehicles to discern relationships that can then be embedded in a larger framework of causation (Lichbach’s suggestion). Instead, integrated comparative analysis treats aggregate analysis and country case analysis as being in a dialectical relationship, each raising questions about the other. In particular, the case studies, using a different logic, problematize the assumptions and findings of the quantitative studies.

Both the political trajectories and integrated comparative analysis approaches, implicitly at least, inject a level of indeterminacy into their explanations. In that sense, much of the new scholarship has stood in contrast to older studies, such as those by Skocpol and Goldstone, whose strict structuralism and emphasis on large-scale forces, at the expense of the agency of local actors and forces and of the interactive effects of more general forces with distinct local

forces, painted overly determined portraits (Goldstone 1991; Skocpol 1979). The new scholarship often also has rejected assumptions of strict rationality, found in scholars such as Bates and Levi, opting instead for an approach that takes into account the unintended consequences of the engagement of the general and the particular (Bates 2001; Levi 1988). In fact, “unintended consequences” might well serve as the mantra for the new research. It follows from Elias’s understanding of historical change: *“This basic tissue resulting from many single plans and actions of men can give rise to changes and patterns that no individual person has planned or created. From this interdependence of people rises sui generis, an order more compelling and stronger than the will and reason of the individual people composing it”* (Elias 1998: 50; emphasis in the original). A number of works in both political trajectories and integrated comparative analysis have thus favored a process-oriented analysis of states rather than a unidirectional causal analysis, a trend that McAdam, Tarrow, and Tilly also point to in this book as evident in other parts of comparative politics and international relations. European scholars, directly, and Americans, perhaps indirectly, seem to have been influenced by Elias’s process-oriented methodology (Mennell and Goudsblom 1998: 36–39). In the next two sections, I will look at the emerging clusters of research using political trajectories and integrated comparative analysis.

Political Trajectories

Even beyond the study of the state, social science generally has witnessed a return to historical analysis over the past decade, often in a methodologically more rigorous guise than in the past, taking sequences of events into account. In Katznelson’s words elsewhere in this book, “Scholars have taken to heart the idea that social science and history cannot be constituted meaningfully without each other” (p. 96, this volume). Probably the most important proponent of this approach has been Paul Pierson:

Contemporary social scientists typically take a “snapshot” of political life, but there is a strong case to be made for shifting from snapshots to moving pictures. This means *systematically* situating particular moments (including the present) in a temporal sequence of events and processes stretching over extended periods. Placing politics in time can greatly enrich our understanding of complex social dynamics. (Pierson 2004: 2; emphasis in the original)

Pierson goes on to complain that much of social science has engaged in a “decontextual revolution,” a phrase I very much like.⁴ “Placing social analysis in time,” he argues, “implies recognizing that any particular moment is situated in some sort of temporal context – it is part of an unfolding social process. Now ‘context’ has become, for many in the social sciences, a bad word – a synonym for

⁴ Rational choice theorists and formal modelers, in particular, have been accused of decontextualizing politics, leading some of them to a reconsideration of some elements of the rational choice approach. See Bates et al. (1998).

thick description, and an obstacle to social-scientific analysis” (Pierson 2004: 167). What Pierson claims about social science generally is particularly true for the study of the state, as scholars have attempted to theorize about it by looking at a frozen frame of its institutions, interests, or other attributes.

Several political scientists, most notably Jean-François Bayart, have begun to develop the concept of political trajectories (Bayart 1991: 51–71), taking seriously the issues of time, sequence, and context. They have set out to show how groups of countries (Bayart is an Africanist) differ in their state practices.⁵ Following the classic work of Braudel (Braudel 1995), Bayart’s emphasis is on the long *durée*, taking special note of the precolonial experience of African states and their institutions.

Political trajectories point to the particular set of world conditions that existed at the time of states’ formation and other critical junctures of their history. And they look at how key elites, social forces, institutions, and conditions mixed in different ways, accounting for the great disparity in state practices. It is the struggle and engagement of distinct local social forces with common global forces, such as colonialism or spreading capitalism, that mark off these new political trajectories works from the sociological-historical research in, say, world-systems theory (Modelski 1987; Wallerstein 1974), and from structural scholarship, such as that of Robert Putnam explaining the differences between northern and southern Italy (Putnam 1993), which both tend to overly determine outcomes and focus on central causal factors, all but ignoring the agency of the periphery.

The political trajectories literature takes note of how institutions develop in ways that often are a far cry from the historical forces that spawned them. A complex of global forces, such as those associated with spreading capitalism, will not have similar consequences everywhere. “The institutional slate is never wiped clean, even in founding moments,” writes Ziblatt, a researcher who uses a political trajectories approach not to study outlaw states but European ones, for which this sort of analysis seems as applicable as for postcolonial African states (Ziblatt 2006: xi). The messy institutional slate is different for each state; nonetheless, historical political trajectories do cry aloud for some broader hypotheses about *groups* of states, regional or otherwise, that formed in similar historical circumstances.

These sorts of hypotheses have begun to emerge in important research that has appeared in recent years on Europe, sub-Saharan Africa, North Africa, the Middle East, Latin America, Southeast Asia, and more. Ziblatt’s own work is a good example, expounding on a recurring theme in the political trajectories literature: the uncertainty and unevenness of central state control over the state’s claimed territory and the varying arrangements between the center and outlying areas. His particular focus is the historical development of federalism

⁵ A U.S. Africanist who has adopted a form of political trajectories analysis is Jeffrey Herbst. See Herbst (2000).

in Italy and Germany (Ziblatt 2006). Catherine Boone's work similarly delves into the varying control of states within their borders (Boone 2003) and also follows the give-and-take between central state authorities and elites in the hinterlands (a recurring theme in a good share of the recent research on the state). Using a blend of Marxist and rational choice analysis, Boone follows the trajectories of Senegal, Ghana, and Côte d'Ivoire as states there established what she calls "uneven institutional topographies" (Boone 2003: 38).

Mounira Charrad focuses on the intersection of state-making in North Africa and women's rights in Islamic societies (Charrad 2001). What forces led to different state policies on family law and women's rights? Why did the state adopt fairly radical personal status laws in Tunisia, thoroughly conservative ones in Morocco, and fairly conservative ones in Algeria (only after attempts at reform there stalled)? Charrad's answer to these questions is pursued largely along structuralist lines, emphasizing how tribes were integrated into the state. The state in North Africa, she argues, has tried to develop authority over society in the context of ongoing contests for loyalty and control with kin groups. Where the state grew independently of the kin groups, as in Tunisia, it could adopt a liberal women's policy; where it developed in concert with the tribes, as in Morocco, it enacted a code upholding Islamic family relations; and where it formed in partial alliance with tribes, as in Algeria, reform-minded state officials aimed at liberal laws but could not sustain those efforts. The book starts with the commonalities of the three countries, proceeds to their precolonial differences, and finally analyzes the differing processes of state formation in the middle decades of the twentieth century.

Some structuralist works, emphasizing the importance of institutions historically, trace political trajectories through path dependence, that is, they look at critical moments when countries pursued one path rather than possible others. These choices, in the words of Mahoney, "establish certain directions of change and foreclose others in a way that shapes long-term trajectories of development" (Mahoney 2001: xi). Mahoney's research on Central America looks at the range of states there, from those that were characterized by repressive military regimes (Guatemala and El Salvador), to traditional dictatorships (Honduras and Nicaragua), and, finally, to democracy (Costa Rica). Borrowing Collier and Collier's (Collier and Collier 1991) notion of the "critical juncture," Mahoney argues that the sorts of choices Central American elites made in the period of liberal reform in the late nineteenth and early twentieth centuries set the course for state-society relations for most of the rest of the twentieth century. "Without exception across the Central American region, national regimes consistent with the structural foundation of the liberal reform period had been created by the mid-twentieth century" (Mahoney 2001: 264). Besides being influenced by the Colliers, Mahoney drew from the earlier work of Barrington Moore and Greg Luebbert, as did some others using the political trajectories approach (Luebbert 1991; Moore 1966).

Like Mahoney, Dan Slater's focus is on elites. His dissertation, "Ordering Power," demonstrates how political trajectories capture the interaction of general forces with specific local conditions in Southeast Asian states. His work

ambitiously examines the political trajectories historically of seven states in the region – Burma, Indonesia, Malaysia, the Philippines, Singapore, South Vietnam, and Thailand (Slater 2005). Noting vast differences among these in the capacity of the states and in the durability of the authoritarian regimes they spawned, Slater asks how and why such differences emerged in the same region among states facing a similar array of forces (Japanese occupation, for example, affected them all) in the same world-historical moment (following World War II). The research attacks the question of state capacity by looking at the ability of states to mobilize revenues in order to maintain themselves and accomplish their goals – in other words, their ability to tax their populations.

Slater argues that differences among the Southeast Asian states are rooted in the political trajectories of their elites and how threatened they were by goings-on in the larger population. In other words, the patterns of elite behavior that developed over time were rooted in the history of internal conflict. Elite unity, or collective action, was fostered, ironically, by high levels of contention among domestic social forces. Such conflict induced elites to work together, especially through broad-based political parties that they formed.

Tracing the timing and type of internal conflicts in the seven countries, especially the degree to which those conflicts physically threatened ruling elites, Slater explains the vast differences in state capabilities, as well as variations in the durability of regimes and regime types. Capable states, such as those in Singapore and Malaysia, emerged where conflict was particularly threatening – seen by the elites as both endemic and unmanageable – and where that conflict exploded before authoritarian regimes were in place. These conditions provided the necessary conditions for new state elites to act together to build effective centralized state institutions. And, where the internal conflicts appeared more manageable – because they were rural or occasional or free of strong communal overtones – elites were not pushed into all the costs associated with forming effective organizations among themselves and, in the end, produced far less capable states, as in the Philippines and Thailand. In these countries, the historical timing was reversed: The threats became far more foreboding after the regimes were in place, rather than before, with very different outcomes in state formation.

Slater's research is part of an emerging set of Ph.D. dissertations in political science employing political trajectories, comparing limited numbers of states, and seeking Merton-like middle-level hypotheses. Prominent among these are works by a remarkable group of students at my own home institution, the University of Washington (Hussin 2007; Kuru 2006; Sezgin 2006; Shin 2006; Ó Murchú 2000). If there is a central concern in all their research, it involves the historical formation of states, which have been deeply affected by the West but have developed along very different political trajectories. Several of these young scholars have looked at cases in which Islam has played a central role in state formation, leading to a very different notion of the state from that embedded in the standard image. Following Boone (Boone 2003), at least some of the newer work understands states' political trajectories to be deeply influenced by the

give-and-take, negotiation, collaboration, and contestation between central state authorities (themselves sometimes fragmented) and dispersed, but locally powerful, social forces. The character, makeup, behavior, and capacity of states in these works stem not from purposive action of state elites, but from the unintended consequences coming from the “dynamic figurations,” to use Elias’s term, involving states and other power holders and seemingly powerless figures in society, all in the context of larger global forces framing their interactions. In these works, neither individual rationality nor institutional logic alone can explain the historical unfolding that occurs; much of the explanation derives from the process of engagement of different sets of purposive actors, with ends that none of these actors sought or anticipated. To my mind, this is a very promising avenue of research. As I will indicate in the next section, that same indeterminacy permeates even some of the research using quantitative analysis.

Integrated Comparative Analysis

Another cluster of research on the state has begun developing a closer relationship between large-N analysis and in-depth case studies, what Benjamin Smith has called “integrated comparative analysis.” The interest in bridging the divide between quantitative and qualitative methods in comparative politics has been growing generally, as evidenced by a 2003 symposium on the topic in the newsletter of the Comparative Politics Section of the American Political Science Department (Anonymous 2003). In one piece in that forum, Lieberman suggests how to develop a “nested research design” (a slight variant on the term “integrated comparative analysis”), involving both large-N and small-N analysis (Lieberman 2003a). He begins by describing “on-the-line” case studies, which employ a fairly standard approach of mixing quantitative and qualitative analysis. And then moves on to “off-the-line” cases, where a more tension-filled relationship exists between the cases and the aggregate analysis.

In on-the-line cases, the researcher uses the big-N study as the touchstone, engaging in causal analysis, usually through regressions, and employs the individual case studies as means to elaborate and deepen the quantitative findings. An example of this sort of research can be found in World Bank studies on civil war, one volume of which is dedicated entirely to African states and another to Europe, Central Asia, and other regions. The starting point is a heavily cited article by Paul Collier and Anke Hoeffler, which came out of the work of the Bank’s Development Economics Research Group. The article develops an econometric model, using logit regression, to predict the outbreak of civil war (Collier and Hoeffler 2004). Spurning standard interpretations based on political and economic grievances, the Collier–Hoeffler model points to the actual opportunities that exist, economically and otherwise, to build a rebel organization to challenge the state. Opportunities include the ability to finance the rebellion (especially through primary commodity exports), the costs of rebellion, the military advantage for rebels that comes from the existence of a dispersed population, and the size (and presumably the heterogeneity) of the country’s

population. The conclusion is that where building a rebel organization to take on an existing state is a viable enterprise – in other words, where the possibilities for financing and organizing it are numerous – rebellion is more likely to break out. While the World Bank sees the issues of rebellion and civil war as “development” problems, they are also clearly key elements in understanding states and the challenges they face, as can be seen in another volume on the subject, which also uses integrated comparative analysis, the recent book by Stathis Kalyvas (Kalyvas 2006). In any case, the World Bank volumes on Africa and other regions, *Understanding Civil War* (Collier and Sambanis 2005), are intended to validate the Collier–Hoeffler model and to deepen it.

Lieberman’s on-the-line cases may do far more than validate the aggregate analysis; they may actually complicate some of the general hypotheses in the causal analysis, setting up a kind of dialogue, even debate, between the quantitative and qualitative data. This sort of dialogue emerges in the work of Kristin Bakke.⁶ Her research, too, was on state relations with potentially rebellious groups. She asked why some states with federal (or decentralized) arrangements managed to come to a modus vivendi with contentious minorities (usually ethnic groups) and others failed miserably, falling into violent civil wars (Bakke 2007). Rather than starting her analysis based on an ideal model of state–society relations, Bakke begins with the wildly different outcomes that have arisen from similar institutional arrangements – in her case, federalism.⁷ Domestic historical conditions and specific circumstances at the moment ensure that a given structure, such as federalism, will not be either universally “peace-preserving” or the opposite.

Indeed, her argument that there is no “one-size-fits-all” federal solution for conflict in states riven with divided societies can be applied more broadly to almost any structural arrangement. Using data from 22 countries, Bakke and her collaborator in the quantitative part of the analysis, Erik Wibbels, demonstrate that the key domestic conditions that determine the ability of federalism to lead to state-minority accommodation are the relative level of wealth in the troubled region and the concentration of the ethnic group in question in the particular territory (Bakke and Wibbels 2006). But the bare-bone nature of these hypotheses – why these conditions are so important in determining whether there will be civil war or peace – does not become clear until Bakke pursues them in her three in-depth case studies: Chechnya in Russia, Punjab in India, and Quebec in Canada. More to the point, her field research began to point to limitations of the big-N findings. The Chechen and Punjab cases, in particular, indicated the importance of unity or disunity in the dissident regions. What did not come out in the big-N analysis was that, when local government in these

⁶ Bakke is another of the researchers who came out of the University of Washington’s Ph.D. program.

⁷ Roeder (2007) offers a variant on the issue of federalism, or decentralization.

areas was not strong enough to bridge the divide among various factions in the minority, violent conflict with the center tended to flare. Also, Bakke found that once conflict achieved some momentum, even when the structural conditions no longer obtained, the violence could continue. In short, on-the-line cases can go beyond validation to a dynamic interaction between quantitative and qualitative data coming out of large-N and small-N parts of the study.

A second sort of “nesting,” according to Lieberman, occurs with off-the-line case studies, which are purposely designed to create such a dialogue between the two sorts of data. Here, in Lieberman’s words, “interesting puzzles that are worth studying almost always involve outcomes that defy our ‘prior’ expectations” (Lieberman 2003a: 18). He quotes Michael Coppedge, who uses Venezuela as an off-the-line case study: “The large-sample study identifies what was likely to happen, and then the case study develops deterministic arguments that move us from what was likely (or unlikely) to happen, to what had to happen” (Coppedge quoted in Lieberman 2003a: 18). In other words, the large-N study is used as a sort of foil, generating broad-stroke understandings of the phenomenon at hand, only to be substantially modified or even jettisoned once one takes a close look at individual cases.

Mary Alice Haddad uses these sorts of off-the-line case studies in her book, *Politics and Volunteering in Japan*, to subvert some common truths coming out of quantitative analysis (Haddad 2007). She examines volunteering, focusing on the importance of community norms, what people collectively believe to be appropriate ways to demonstrate their civic responsibilities; indeed, the community, not the individual, is her basic unit of analysis. These community norms, she theorizes, are products of people’s ideas – what they think are the correct ways for individuals and states to deal with social problems – and of the actual practices of states and social institutions, which promote or inhibit volunteering. Her approach is to treat an aspect of civil society, volunteering, as a product of distinctive relationships between individuals and their organizations, on the one hand, and state institutions, on the other. She sets out to explain inter- and intracountry differences in rates and nature of volunteering by looking at both state and society, as well as how they interact with one another.

The United States looms large in Haddad’s study, even though the primary research is on Japan. Starting with some broad empirical projects on volunteering – the Comparative Nonprofit Sector Project and the World Values Surveys project – she finds, based on her study of Japan, that the quantitative research coming out of these works, while very valuable in many regards, has been systematically biased in favor of certain types of volunteering, precisely the sort found in the United States. “Overlooking embedded volunteer organizations in comparative studies of civil society has created a *systematic bias* in favor of certain types of organizations, in particular, the types of organizations found in the United States, and a bias against the types of organizations found in many other countries in the world, Japan in particular” (Haddad 2007: 42; emphasis in the original). By “embedded,” she means social organizations that have a close relationship with the state, including frequent, habitual relationships with the

bureaucracy in which they work together on policymaking and implementation. Indeed, what struck her upon setting foot in Japan and what was confirmed in her empirical research are how much higher voluntarism of certain sorts is in Japan than in the United States, in direct opposition to what the quantitative studies had indicated.

Similarly, Benjamin Smith uses off-the-line case studies to question prevailing wisdom coming out of quantitative studies (Smith 2007a) on oil as a factor in political stability. Oddly, the “truths” that he grapples with are contradictory: (1) political instability is a sure result of oil wealth and (2) authoritarian regimes gain strength and stability from oil wealth, which gives them resources for patronage and repression. Conducting his own quantitative analysis, Smith uses 107 countries to estimate the effects of oil wealth – more specifically, the sudden contraction of cash coming from oil – on regime failure, antistate protest, and domestic armed conflict. He finds, first, that the oil-rich states in his sample had amazing durability but not, as some of the literature indicates, due to pouring their money into a repressive apparatus. Even as oil states faced rapid drops in income when oil prices plummeted in the mid-1980s, most of their regimes seemed to fare remarkably well, avoiding threatening political crises. Smith shows how some of the “truths” had come out of hypotheses generated by too few cases or by big-N studies that had serious methodological problems in linking political instability and other factors. Combining elements of political trajectories analysis with sophisticated quantitative analysis, Smith castigates these earlier studies for “assigning preferences *ex ante* and studying institutional change as a matter of comparative statics – comparing data points A and B and inferring what took place in between – [which] misses a large part of the picture: how those preferences come to be formed over time by the experiences of development, state building, and subsequent patterns of institutional change” (Smith 2007a: 50).

Smith spurns the determinism found in studies that see oil (and all that is associated with it) as leading to political instability *or* political longevity. He rejects a universal model – in this case, that the global oil market creates a single “set of political constraints common to all exporting countries” (Smith 2007a: 196). Smith focuses on how some leaders in oil-rich states invested, not in repression, but in complex state institutions and political organizations, which could carry them through even lean years. But not all leaders did this. Comparing Indonesia, where the regime survived the drop in revenues in the 1980s, and Iran, where the regime fell, he demonstrates how historical timing influenced outcomes. In Indonesia, the regime started without oil revenues, which came online only later, and leaders felt compelled to build institutions, especially tax-collecting ones, that strengthened the state and the regime’s relationship with, and control over, the population. The shah’s regime in Iran, in contrast, had bountiful oil revenues from the outset and felt little compunction to build such institutions, paying the price for that decision in the Iranian Revolution.

Smith’s research does not choose qualitative over quantitative analysis, nor does it privilege historical-institutional (or structuralist) over rational choice approaches (Smith 2007a: 197). His analysis is syncretic: The different

“approaches provide much more explanatory bite than either can alone” (Smith 2007a: 197, 202). But, like Bakke, he insists that one size does not fit all: “Oil and oil wealth do not carry around one set of incentives and constraints that hang over actors in all times and all places” (Smith 2007a: 198). That eclecticism and unwillingness to pigeonhole states into ill-fitting models or preconceptions have become hallmarks of the new research on the state.

TOWARD A MULTILAYERED, MULTIPARADIGM ANALYSIS OF STATES

Since at least the end of the Cold War, state leaders have had to operate in a global environment in rapid flux, which has, in turn, made for a quick-changing domestic scene as well. More and more of the state’s functions have been assumed by private domestic and transnational organizations; globalization has also hit hard at the kind of territorialism associated with states in the past; and world conditions seem to have enabled growing state-minority conflict and violence. At the same time, in the decade or so since the first edition of Lichbach and Zuckerman’s *Comparative Politics* appeared, big strides have been made in the study of the state by social scientists. The determinism, the one-size-fits-all structural analysis, and the assumptions of a linear relationship between preferences and outcomes found in many earlier works have all given way in much of the research, including that based on political trajectories and integrated comparative analysis.

In the past 10 years, work in comparative politics has struggled to maintain the universality of the concept of “state” itself, that standard template, while dealing with the extraordinary diversity of actual states and the very different conditions under which most of them emerged in the late nineteenth century and the twentieth century. As a group of European social scientists put it in the conference program for a 2006 workshop sponsored by the Danish Institute for International Studies, the Humboldt University (Berlin) Institute for Social Sciences, and the Max Planck Institute for Social Anthropology, “A variety of catchy headings such as state failure, weak states, or good governance indicate attempts to come to terms with the seemingly growing complexity of political authority on a global scale” (Anthropology, Groups M.P.I.F.S, and Studies 2006); these “catchy headings” point to the limitations of funneling comparative analysis exclusively through single standards of the idealized Western state, or any other standard, while still recognizing the existence – at the very least, in ordinary people’s minds – of a single genus of the modern state.

The new clusters of research, such as political trajectories and integrated comparative analysis, have not taken a single, straightforward analytic path. But much of the recent research has accepted the uncertainty of outcomes – unintended consequences – shunning the deterministic analysis that had crept into some of the earlier rationalist and, especially, structuralist works (Bates 1981; Goldstone 1991) and moving instead to a much more open-ended analysis, contingent on historical timing and conditions. And it has reintroduced the “political” into the study of the state, focusing on the struggles between

competing forces in the state and in society, often between central state elites and far less visible social forces in the hinterland who hold unexpected power, as well as the uncertain results of those struggles. Again, in the words of the European social scientists in their conference program, “We want to address the importance of the social practices of individuals, collectives, organizations, of state and non-state actors, and the intended and unintended outcomes of their interactions. . . . The resulting formations of governance sometimes look like an approximation [of] the Western ideal type of statehood, and sometimes they might not. The important question seems to us how these systems actually work and what effect they have on the dynamic of governing.” They highlighted the existence of what they call the “different constellations of rule” (Anthropology, Groups M.P.I.F.S, and Studies 2006: 1).

While researchers from North America, on the whole, have been, perhaps, less self-conscious than their European counterparts about the move from the “Western ideal type of statehood,” their work, too, has moved the comparative politics of the state away from that single standard while still grappling with state similarities that span the globe. Drawing from strands of research on the state in recent years, I want to suggest a tri-level focus as a starting point for analyzing states, which (1) takes account of universal state similarities and common forces that all of them face, (2) creates typologies allowing for the comparison of small subsets of states that are akin to one another, and (3) highlights the particular ways political elites and ordinary people in any society face challenges and exploit opportunities – and run headlong into each other as they scamper to minimize risk and maximize gain.

- 1 *The image of the state:* As one looks at the 200 or so cases that constitute the twenty-first-century gaggle of states, it is impossible to ignore certain common elements. All, or nearly all, have flags, currencies, and postage stamps of their own. They all have police forces sporting spiffy uniforms, and practically all have uniformed armies, too. Every state has a legislative body and courts, in addition to some sort of executive. No state lacks public schools and a state treasury. The list goes on. John Meyer and his colleagues have rightly insisted on the importance of this near-universal recurrence of forms and symbols, what they call “structural isomorphism in the face of enormous differences in resources and traditions” (Meyer et al. 1997: 145). While I think that they tend to assume too quickly that the isomorphic institutions of states actually do what they were set up to do or claim to do, their notion of a worldwide model is an important one in analyzing any state, today or in the past. And, beyond the structural isomorphism, it is also the case that populations the world over have developed common understandings about what states are supposed to look like and what they are supposed to do, even if practices fall far from their hopes and expectations. As noted, I have called all this the image of the state. The notion of image implies that the state is imagined – that a state isn’t fully a state, in the words of Strauss and

O'Brien, until it is popularly imagined (Strauss and Cruise O'Brien 2006: 5) – which, in turn, suggests the need for culturalist tools to understand that imaginative side. Meyer and his collaborators have used the term “worldwide model” and similarly pointed to a cultural element that they call “world culture.” Still, their analysis is largely structuralist, emphasizing institutionalist tools to understand the model. Indeed, analysts who have incorporated some notion of isomorphism of the state into their analyses have used mostly a structuralist approach. Whether one calls this overall isomorphism, the image of the state, the worldwide model, or anything else, it is essential to include this universal element, with its globally recurring features, pressures, and popular expectations, into any research design comparatively studying state formation and operation.

- 2 *Structured comparison*: Beyond this universal component, comparative political analysis needs to break down the overall genus of the state into more manageable species and then, again, into subspecies. Some of the new research on the state, for example, has used the categories first prompted by Gerschenkron, separating states by their role in development, late development, and late-late development (Gerschenkron 1962; Smith 2007a). The preceding discussion on the groups of states that emerged under similar world-historical conditions, such as the subsets of new states created after the two world wars, also point to temporal conditions that, in part, accounted for the dynamics of different sorts of states. Similarly, broad regional groupings, such as Latin American and African states (Hanson forthcoming), or ones with certain endowments, such as oil (Ross 2001; Smith 2004), imply a typology that differentiates among extant states. Practically all the works I mentioned earlier in the discussion of political trajectories and integrated comparative analysis further narrowed the comparison of states by looking at anywhere from two to seven states, a kind of subspecies. Structured, focused comparisons most often involve comparisons within a region but, increasingly, comparisons are being made on the basis of other factors, too, such as the structure of the state (e.g., federalism) or states coming out of the same colonial empire (Lieberman 2003a: 19). Not surprisingly, these sorts of groupings of states have resulted, at this level, too, in works emphasizing structural analysis, particularly using institutionalist tools, over rationalist and culturalist approaches.
- 3 *Domestic practices*: Finally, analysis of states is impossible without close attention to proximate causes and conditions, without carefully understanding how larger global, regional, or other forces or pressures, as well as tensions originating inside the state's boundaries, work themselves out in specific cases. To be sure, a strong structuralist current runs through the analysis of specific countries' state institutions and how they function. These institutions “take shape according to the circumstances surrounding their creation” (Smith 2007a: 200). The contingencies and conditions that actors face in state

formation or in dealing with specific crises certainly shape the options open to them. Options are not unlimited. What those actors do in the context of these contingencies and conditions, which options they choose, can be usefully analyzed, in part, through a rationalistic approach. State officials, like others, make economic determinations in selecting options from among the opportunities and constraints they face. Outcomes, though, are most often not simple linear extrapolations of leaders' rationality under constrained conditions. Unintended results can come out of intentional, purposeful behavior, especially when different purposeful actions by various social forces clash head-on. This paradoxical mix of rationality and indeterminacy, as noted earlier, constitutes Elias's notion of process. Without deep research at the ground level, it is impossible to understand the actual practices of states, which can vary widely, even among states in the same species or subspecies.

Research on the state in the past decade, such as that found in political trajectories and integrated comparative analysis, has tended to mix and match approaches, using structuralism, through a variety of institutionalist tools, as well as rationalism, usually through microeconomic, or rational choice, tools. Regrettably, culturalist approaches have been much less in evidence, except in single-case interpretivist studies. The major thrust of most of the structuralist/rationalist works has been on the first face of the state mentioned earlier in this chapter, the hard face of domination and social control. Allegiance and representation, the second face of the state, has received less attention in state research. It is precisely this aspect of the state – its softer side – that would benefit from a cultural perspective.

In my chapter in the first edition of this volume, I quoted two statements about the study of culture and the state, which seem nearly as true today as they did a decade ago: "In every way," wrote Archer, "'culture' is the poor relation of 'structure'" (Archer 1985: 333). And Laitin added, "The systematic study of politics and culture is moribund" (Laitin 1986: 171). I indicated then, and still believe, that the study of culture and the state could yield fascinating results in the comparative politics of the state. This statement holds particularly true in trying to understand the diversity of state practices aimed not so much at social control as at gaining legitimacy, at giving the population a sense of membership in and belonging to the state. As I argued, performative rituals associated with the state, often undertaken as ends in themselves rather than simply as means to increase power, can go far in uncovering the basis for divergent state practices.

Culture and the master narratives they generate can explain some of the enduring practices of states even in the face of seeming state dysfunctionality in issues of governance or control. Master narratives – often founding stories – are both *constructed by the state* and help to *construct the state*. These narratives, or shared stories, as Ross writes elsewhere in this book, refer to the commonly held intersubjective meanings people hold about who they are and their relationship to key institutions, including the state. Indeed, if Lichbach and Zuckerman

are right in the opening chapter that constructivism is a gathering storm, comparative politics can expect to see more on how narratives interact with and construct the state. While 10 years ago my examples of this sort of theoretically oriented cultural analysis of the state came mostly from outside political science (e.g., Geertz 1980; Thompson 1974), recently a handful of political scientists have gone beyond single-case interpretivist work to theoretically sophisticated studies of culture and the state. One of the best examples is in Strauss and Cruise O'Brien's edited book, theorizing about the politics of emotion and drama, what they call the "politics of performance," and focusing on trials, elections, and contestation from below and public space. Their focus is on "categories of affect, emotional engagement, and imagination" (Strauss and Cruise O'Brien 2006: 2–3).

They maintain that the symbolic element, both by the state and by individuals engaging the state (even trying to topple it or appropriate it), comes through three sorts of performative elements: "state ritual (such as staged ceremonies), theatre with semi to totally open ended outcomes (such as elections and street demonstrations), and individual performance or micro-performance (a particularly dramatic speech or event designed to engage the emotions and rally support)" (Strauss and Cruise O'Brien 2006: 3). The symbolic dimension is evident in the "tropes, symbols and metaphors presumed to be well understood by those audiences" (Strauss and Cruise O'Brien 2006: 4). Performance makes the state visible to the population and transforms well-worn symbols from other dimensions of life, such as religion, aiming to lead a broad audience to identify with the state (Esherick 2006).

Lauren Basson pursues another cultural dimension in studying the state, in her case the American state, by focusing on the meaning of membership in nation and state (Basson 2008). As Ross makes clear, these meanings and meaning-making are central to explaining how people understand their world and their relationship to political institutions. He talks about culture providing people with political and social "identities," while I prefer to think of culture as creating a sense of "belonging" (Migdal 2004). Basson treats meaning in terms of membership in both the nation and the state; she thus connects to a host of other publications in the past decade based on the renewed interest in the concept of "citizenship." Basson's approach takes the reader not to the capital city, the center of the state, but to its margins or boundaries, where changes in the meaning of state and nation have often been initiated. Indeed, she argues that outside-in analysis, what Solingen in this volume calls "Type A effects," gives the researcher new leverage in understanding the state. Starting with how the nation and state were imagined in the late nineteenth century, as grounded in a homogeneous, racially pure, white citizenry, Basson follows a number of *causes célèbres* in which indigenous people of mixed descent and those in mixed-race territories challenged the prevailing image of what the American state was and what it meant to be an American. To her mind, states build clearly defined categories of rights and responsibilities, differentiating among segments of their populations, including various sorts of citizenship. Change occurs when certain people – such as those of mixed race – do not fit into any of those fixed categories

and challenge the state's basis for classification of the population. Her cultural approach, mixed with the use of institutionalist tools, offers important new insights into the state's relationship with its population.

Studying the state involves probing a multilayered, multipurposed entity, whose parts frequently work at cross-purposes. And this complex state operates in a similarly complex multitiered environment, which deeply affects the state and, in turn, is affected by the state. The complexity begins with the two faces of the state, that signifying control/power and that pointing to representation/loyalty. Second, the complexity continues with the overall set, or genus, of states, which demonstrates amazing isomorphism, or universality, while at the same time including just as amazing diversity – the single *image* and multiple *practices* found within the set of states. Finally, the complexity includes at least three analytic tiers of forces acting in a reciprocal relationship with states: the common global forces all states face; transnational or international forces, such as colonialism, regional factors, and resource endowments, interacting with a distinct subset of states; and the unique domestic cultural and social forces each state encounters.

All this complexity has turned the experience of researching the state into an eclectic enterprise. It demands a full toolkit – an amalgamation of culturalist, structuralist, and rationalist tools and of historical, case, and quantitative methods. This seemingly banal observation follows Lichbach's point in his chapter of this book that the juxtaposition of several paradigms can lead to new ways of looking at things. Different perspectives highlight the variegated visages of the state and their interaction with their domestic, regional, and global environments. Many, though certainly not all, political scientists studying the state have abandoned singular approaches of rationalism, structuralism, or culturalism; they have begun to shy away from high-level generalizations, which decontextualize the state; they have migrated from linear, causal models toward process-oriented analysis; and they have gone from comparative statics to historical analysis, emphasizing the importance of the temporal and of sequencing. To my mind, all these changes have made for a comparative politics of the state that has asked more interesting questions and come up with richer, more satisfying explanations.

An Approach to Comparative Analysis or a Subfield within a Subfield?

Political Economy

Mark Blyth

INTRODUCTION — AND A FEW CAVEATS

The first edition of this volume featured a chapter on political economy by Peter A. Hall (Hall 1997). In it, Hall sought to define political economy by asking the following question: In the subset of those scholars who study the comparative politics of the advanced industrial states, how are political economy explanations constructed? Seen from this vantage, political economy, Hall answered, appears as a field defined by a specific set of concepts; interests, institutions, and ideas within comparative politics. Ten years later, I still find Hall's specification of the boundaries of the field, by reference to this troika of concepts, to be most useful for defining what political economy is and what it is not. However, in replicating Hall's analysis, two caveats are in order.

First of all, in following Hall's troika of "interests," "institutions," and "ideas" as defining political economy, I necessarily break with this volume's emphasis on "rationality," "culture," and "structure" as defining comparative politics. Why then do I prefer Hall's troika of boundary-setting concepts to the one offered by the editors? I do so since taking this route allows me to focus on interests rather than rationality as one of the three defining concepts of the subfield. This positioning is helpful, I suggest, insofar as while many political scientists see rational choice and political economy as synonymous (Weingast and Wittman 2006; Alt and Shepsle 1990), a focus on interests rather than rationality as a core concern allows me to place political economy in a broader frame that engages a more variegated set of literatures, particularly those on institutions and ideas, than would at first blush appear to constitute it. When one makes this distinction, political economy does not become coterminous with rationality, and hence with rational choice theory. Rational choice is masterfully surveyed in this volume by Margaret Levi, and indeed, much of the work she covers is and should be considered political economy. However, in terms of the framing of this chapter, if rational choice gets to "own" the concept of rationality, then much of the work I would wish to include here must be recoded as some kind of cultural or structural "other than political economy" approach. Rather than privilege any particular literature, Hall's framework allows us more freedom to broadly define and survey political economy.

Despite having just made the case for the usefulness of these concepts in defining the boundaries of political economy, my second caveat concerns how developments in the subfield over the past decade may have actually made sustaining these boundaries more difficult for two reasons. That is, while political economy is indeed still very much a way of doing comparative politics, one can argue that it has grown from its “reborn” beginnings in the 1970s to become a distinct “way of doing” political science in and of itself. For example, as well as scholars working with interests, institutions, and ideas developing their own research programs, we have also recently seen the decay of the boundary that traditionally set so-called international political economy apart from comparative political economy, increasing conceptual borrowing across the subfield, and the emergence of new approaches that go beyond the troika of interests, institutions, and ideas (Hobson and Seabrooke 2007; Langley 2004; MacKenzie 2006).

With those caveats in order, I first discuss what political economy is and how its modern form came about. Following this, I suggest that the modern troika of political economy perspectives just outlined can be read as extensions of three books published almost a quarter of a century ago: Peter A. Gourevitch’s *Politics in Hard Times* (1986), Peter A. Hall’s *Governing the Economy* (1986), and Peter J. Katzenstein’s *Small States in World Markets* (1985). I link each of these books to one part of the troika; interests, institutions, and ideas, respectively, and show how the evolution of each of these perspectives can be understood as an extension of the core insights of each of these classic statements. In noting this increasing plurality of approaches, however, I assess two things. First, I return to the relationship of political economy, as it is bounded here, to rational choice theory. Second, I ask whether the plurality of approaches this broad view of political economy establishes is a problem for a putatively scientific field of study. Comparative politics, as a subfield, is sometimes criticized for its intellectual pluralism (Skocpol et al. 1995; Bates 1997b). I argue instead that as far as political economy is concerned, such pluralism is its signal strength. When one considers that the objects of political economy are open-ended evolutionary social and economic systems, the idea that a “theory of everything” can help us understand the world has surely proven fallacious (Blyth 2003a, 2007). As such, the increasing pluralism of political economy should be embraced rather than rejected.

POLITICAL ECONOMY: WHAT IT WAS AND HOW IT (UNEXPECTEDLY) CAME ABOUT

The answers to these questions are not as obvious as is typically portrayed. What one might call the “standard story” is one where the birth of capitalism in Europe transforms property relations and patterns of distribution, and “political economy” thinkers theorize about it (Heilbroner 1953; cf. Halperin 2003). Reflecting upon these momentous changes, thinkers such as Smith, Malthus, Ricardo, and Marx explained these events by reference to factors as different as the division of labor, changes in population, the “average” level of profits, and

the unfolding of class contradictions, respectively. Despite their ostensible differences, what these classical political economists agreed upon – and this is the correct part of the standard story – was the essential unity of the economic and the political as equal components in understanding the way the world works (Watson 2005).¹ However, such unity was not to last into the modern era, and here lies the less well-known part of the standard story.

Later political economists, such as Jevons, Walras, and Marshall, separated economics from political economy during their “marginalist revolution” by focusing on the moment of exchange rather than the process of value creation. But in making such a move, they did something else: They removed from political economy a concern with distribution – who gets what, when, where, and why – and hence any notion of politics and economics as mutually constitutive. In this reductionist moment, Pareto efficiency trumped political expediency, and “the politics” of political economy was evacuated. The result of this separation was that economics forgot history in the search for timeless generalizations about a field of action called “the economy,” where politics appears only as a distortion of (or a distraction in) an otherwise self-regulating world (Hodgson 2001). Although there were some holdouts to this view, such as Veblen (1899) and Kalecki (1944), the early years of the Cold War effectively put paid to political economy, at least in the American academy. In part because it smacked of Marxism, but also because the move toward ever greater formalism further “depoliticized” the subject, economics ascended as a legitimate field of inquiry during a period in which the social sciences as a whole were politically suspect, and as a consequence, “political” economy dropped out of view (Samuelson 1997; Amadze 2003).

The (Unexpected) Rebirth of Political Economy

With economics ascendant and political economy out of fashion, it would take a shock to the system to bring it back in. That shock came in the 1970s, when the gloss came off the post-World II economic boom and the developed world underwent its first postwar recession (Lindberg and Maier 1985; Krugman 1994). Stagflation, oil shocks, unemployment, and low growth all dented the prestige of economics since economics, as a discipline, didn’t see it coming, and economists as a group couldn’t agree on what to do about it once it came (Bell and Kristol 1981). In this moment of crisis, many scholars began to argue that the separation of politics and markets that economics instantiated was perhaps part of the problem all along.

First out of the block were Marxist theorists, eager to denounce the latest dip in capitalist performance as “the end of times.” James O’Connor (1974) diagnosed the postwar welfare state as being in a terminal fiscal crisis, while Jurgen Habermas (1975) identified a parallel “legitimation crisis” in which the overstressed mechanisms of state redistribution worked against efficient mechanisms

¹ One need only reflect that Adam Smith’s *Meisterwerk* is called *The Wealth of Nations*, not *The Wealth of Individuals*, to see this.

of capitalist exploitation. In other parts of the academy, liberal theorists such as Charles Lindblom (1977) pondered the “privileged position” of business as a political actor in capitalist states.² Meanwhile, other scholars began to wonder if it wasn’t democracy that was the problem, with politicians meshing their electoral cycle to the mechanisms of state intervention to produce inflation and low growth through a “political business cycle” (Nordhaus 1975; Lindbeck 1976; Buchanan and Wagner 1977).

Political scientists embraced these new ideas and pondered, for example, which states would survive the downturn better than others, and in doing so began to see the organization of the state itself as a critical variable in explaining economic outcomes (Katzenstein 1976; Krasner 1976). Building upon these insights, some scholars sought answers in analyzing how a post-Bretton Woods financial environment would impact states (Block 1977; Strange 1970). Others worried more specifically about the United States in its role as world hegemon and provider of “global public goods” (Kindleberger 1973). From this angle, an entire body of work concerned with the stability of the global economy and the role of the United States therein, a distinct international political economy (IPE) began to take shape.

Indeed, by the mid-1980s, political economy, in a multiplicity of forms, was back. It was back in part because the excision of the political from the economic that economics as a field relied upon had seemingly failed. It was back in part because with states around the world appropriating, taxing, and spending between 30 and 50 percent of gross domestic product (GDP), the idea of an “economy” that ran by its own transcendental laws apart from politics became an increasingly questionable assertion. Most importantly, however, it came back because hard-won empirical research showed that the economy was inseparable from politics. Modern political economy showed that if one wanted to understand significant variations in economic outcomes, then embracing the mutual imbrications of states and markets was a pretty good place to start.

INTERESTS, INSTITUTIONS, AND IDEAS AS EXPLANATORY ALTERNATIVES

Concepts and Questions

Despite their desire to place markets and states in the same equation, what this new generation of political economists had in common were more questions than answers. The question some scholars asked was “Qui bono?” – who benefits? (Gourevitch 1986; Strange 1988). This question privileges the concept of “interests” by forcing the analyst to ask, “In whose benefit would it be for outcome X to pertain over outcome Y?” Doing so, in turn, leads the analyst to ask how people come to want what they want, and then try to link those wants to specific outcomes worth explaining. Given this desire to link intentions and outcomes, interest-based arguments tend to be underpinned by a materialist

² Lindblom was by training an economist, not a political scientist.

theory of action, based upon the not unreasonable notion that where one sits economically may guide one's preferences politically.³ Actors' class positions, what assets they have, how fungible those assets are, how exposure to particular economic shocks impacts agents' resource portfolios: these variables become empirical grist to the theoretical mill of interest-based accounts (Block 1977; Frieden 1991b; Rogowski 1989; Swenson 2002).

A second group of theorists asked another question: "Who varies and why?" Here institutions rather than interests come to the fore. One influential definition of institutions views them as "the formal rules, compliance procedures, and standard operating practices that structure the relationship between individuals in various units of the polity and the economy" (Hall 1986: 19). Another defines institutions, more broadly, as "humanly devised constraints that shape human interaction" (North 1990: 3). Basically, institutionalists want to know whether agents act according to their materially derived interests or because of the institutional context in which they find themselves. Institutional explanations focus our attention on how economies are organized and how such configurations impact agents' interests. Seen in this way, institutions link larger economic-structural changes and interests but become causally important in their own right. There are, however, two versions of institutionalism that have evolved from this common position that institutions matter.

The first version, as Hall's definition suggests, sees institutions as historically specific and ontologically prior to the agents who occupy them. Such institutions *structure agents' choices* (Hall 1986; Steinmo et al. 1992). North's quote, in contrast, suggests a different but equally relevant way of viewing institutions. Rather than institutions structuring choices, humans are seen to design institutions to achieve their goals given their preexisting material interests. Whether to overcome collective action problems or reap gains in trade, *institutions are chosen structures* (North 1990). In either case, while interests are important, it is how they are refracted through institutions that is the explanatory *causa prima*.

Building on these prior positions, a third group of political economists asked another question: "Who constructs?" Given that many of the causes that generate outcomes in the political economy are highly complex and not directly observable, some theorists began to attend to the social constructions agents use to decode and navigate the political economy, seeing those constructions as causally important in and of themselves (Blyth 2002; Parsons 2003). Take globalization, for example. Standard materialist accounts of globalization's effects on politics tend to focus on how particular actors are impacted by, for example, greater integration into financial markets (Frieden 1991b, 2005), labor markets (Rodrik 1997; Iversen 2005), or product markets (Keohane and Milner 1996b). In contrast, scholars who attend to the social construction of globalization ask instead how the multiple causes of the global economy come to be known as having such specific and unusually linear outcomes in the first place. They point out that what globalization "is" is itself constructed differentially across nations. In the United Kingdom

³ Hence the common locution, "an agent's material interests." See Blyth (2003).

and the United States, globalization is portrayed as an imperative to be embraced. In France and Germany, it is seen instead as a political choice that, when embraced, undermines national conceptions of welfare (Hay and Rosamond 2002; Schmidt 2002). Such scholarship points to something important: How particular constructions of the political economy agents develop and deploy helps bring into being that which is described, rather than simply describing an already existing state of affairs (MacKenzie 2006). That is, how agents think about, and hence act, in the political economy is causally important.

Clearly, such a variety of perspectives invites a plurality of methodological stances. Rather than adjudicate the one “true” version of political economy or suggest that a single approach actually covers all the bases, in the next section I show how these three strands of modern political economy can be usefully understood as the outgrowth of three particular works that all sought to explain the same thing: the economic policy choices of states. These texts can be seen as the springboards for our troika of approaches. Following this discussion, I address the relationship of this version of “what political economy is” to rational choice theory, a body of work that not only has a strong lineage *in* political economy but sometimes aspires to define itself *as* political economy.

Interest-Based Political Economy: Origins

Gourevitch’s *Politics in Hard Times* (1986) is in many ways the touchstone for contemporary interest-based explanations in political economy. For Gourevitch, interests are the primary explanatory concept, and in this regard he is thoroughly materialist. As he puts it, “what people want depends on where they sit” (Gourevitch 1986: 56). However, although Gourevitch seeks to reduce politics to the materially derived preferences of social actors, the way he does this shows an affinity with other approaches that later works in this tradition eschew.

Gourevitch explains how changes in the international economy impact a state’s “production profile” – the configuration of its economic sectors – and alters the preferences of domestic actors, given their assets and resources within such sectors. For Gourevitch, changes in the global economy create moments of political crisis that alter agents’ preferences, and into this breach enter politicians who act as “brokers” who attempt to forge new coalitions of common interest. These coalitions then seek electoral or other forms of power in order to advance public policies to benefit themselves, rather than other contending coalitions whose assets are differentially affected by the same changes (Gourevitch 1986: 55–60, 32–34). Examining the economic crises of the 1870s, 1930s, and 1970s, Gourevitch shows how these variables and causal mechanisms produce political coalitions that vary as his theory predicts.

What makes *Politics in Hard Times* particularly interesting, however, is how Gourevitch’s key category of material interest is actually bound up with, and only understandable through, a host of secondary variables. Specifically, Gourevitch notes how exogenous economic changes rarely, if ever, telegraph into agents’ heads “what has gone wrong” and “what should be done.” As such,

political parties, the institutional configuration of the state, the economic ideas mobilized by agents, and even “a country’s placement in the international state system of political-military rivalries” all come into play (Gourevitch 1986: 21). Indeed, one of the most striking findings of Gourevitch’s book was that as one moved temporally from one crisis to the next, the role of such secondary factors became *more* important in explaining outcomes over time (Gourevitch 1986: 227–228). Yet, despite this, the take-home message was that to explain states’ policy choices, materially derived preferences, political brokerage, and coalitional politics were what should be attended to.

Institutional Political Economy: Origins

A contemporaneous text that sought to explain state policy choices by reference to institutions rather than interests was Peter A. Hall’s *Governing the Economy* (1986). Hall’s puzzle was that if Britain had been in relative economic decline since 1913, and if agents’ interests were in the driving seat, then why did none of these agents seem to have an interest in arresting this decline? Or even if they did, why were they unable to do so? For if political parties are brokers of broad social coalitions, then Britain has seen an alteration in governing parties and coalitions but little alteration in economic policies (Hall 1986: 26–37). Hall’s answer was to see agents’ interests as mediated by their institutional position rather than being telegraphed straight from their structural location.

In brief, Britain was the first industrializer, an imperial power, and a financial center, all of which led to an economy that was an agglomeration of small firms with fractious labor–management relations, small banks, and externally oriented capital flows (Hall 1986: 37–47). While this set of institutions worked well in the nineteenth century, once other countries caught up and surpassed the United Kingdom in the early twentieth century, these institutions became increasingly dysfunctional. However, since such institutions served as the context in which policymakers’ choices were made, *they structured agents’ choices rather than being themselves objects of choice*; key actors’ interests became a derivative function of their institutional rather than their material position. In short, British politicians’ efforts at reform were thwarted by their particular institutions. Like Gourevitch, however, Hall also relies upon other secondary variables as part of his explanation. After all, how do institutions structure choices? Hall says that they do so by processes of social learning such that “off-the-path” policy thinking, and hence radical policy choices, remain off the table (Hall 1986: 233, 277). Institutions may be granted analytic primacy, but Hall sees them as acting on and through economic ideas, political parties, and even the mass media (Hall 1986: 277–280).

Constructivist Political Economy: Origins

A third major contribution was Peter J. Katzenstein’s *Small States in World Markets* (1985). Katzenstein sought to explain why, in the aftermath of the economic shocks of the 1970s, it was the smaller and more vulnerable European

economies that seemed to better weather the economic storm. What makes *Small States* particularly important was that it embedded one explanation (a materialist-coalitional one) in another (institutional) that, in turn, opened the door to other approaches (ideational) that attempt to transcend both. Katzenstein's puzzle was that while large economies adjusted to external shocks either through the market (via wages and prices) or through the state (via policy interventions), given their relative openness to the international economy, small states had to find other, less destructive ways to adjust (Katzenstein 1985: 24–27). That alternative was flexible adjustment through domestic institutions, specifically corporatist institutions, where peak organizations and the state shared the costs of adjustment among encompassing coalitions of social partners (Katzenstein 1985: 30–38).

On the one hand, *Small States* was a straightforward materialist IPE story.⁴ Given their degree of openness and exposure to the international economy, these smaller European states, through processes of domestic bargaining, developed two mutually supporting sets of institutions: corporatist intermediation and welfare compensation. This occurred as exogenous economic shocks altered agents' interests such that coalitions favoring compensation won the day and built institutions to serve their interests. But on the other hand, this Gourevitchian story was a bit too neat and functionalist for Katzenstein. After all, why compromise? Why not conflict as different coalitions push the costs of adjustment onto one another, as happened in the larger European states in the 1930s?

Herein lies the deeper *institutional* version of events that actually drives the argument as a whole. The lack of a strong feudal past and the consequent weakness of the landed aristocracy, as far back as the Late Middle Ages, sets the scene for compromise (Katzenstein 1985: 159–160). Given this *longue durée*, evolving links between industrial and agricultural sectors, the emergence of proportional representation, and late industrialization all combined to favor compromise rather than conflict among business and labor groups by the time we get to the critical period of the 1930s (Katzenstein 1985: 171, 174). It is this domestic long-run historical and institutional development that drives the ostensible materialist-coalitional story. Each alone is insufficient as an explanation, but together they have more explanatory reach.

Yet, *Small States* also suggests a move beyond interests and institutions as explanatory categories, noting, for example, that the “first trait” of such economies was “an ideology of social partnership” (Katzenstein 1985: 32). Motivating such patterns of compromise was

the perception of vulnerability . . . which . . . generated an ideology of social partnership . . . [and] . . . acted like a glue for the corporatist politics of the small European states. . . . Yet none of the reviews of the book published after it appeared paid any attention to it. Why? A decade before the constructivist turn in security studies and international relations, scholars of comparative and international political economy simply did not know what to do with ideology as an explanatory construct. (Katzenstein 2003: 11)

⁴ Especially if one reads chapters 1, 2, and 5.

This is why *Small States* is more than a synthesis of coalitional and institutional approaches. It also opened the door to moving beyond them, to put ideas and social constructions front and center in political economy explanations rather than treat them as a residual category to mop up variance unexplained by these other approaches (Blyth 1997).

EXTENSIONS OF INTEREST-BASED POLITICAL ECONOMY

Trade, Mobility, and Politics

Taking *Politics in Hard Times* as our starting point, we can trace the development and refinement of interest-based approaches in political economy.⁵ Eschewing the richness of Gourevitch's analysis in favor of parsimony and predictive power, one strand of work that emerges from this focus on the effects of the international economy on domestic politics is Rogowski's work on shifts in trade patterns and their effect on domestic political alignments (Rogowski 1989). Using Heckscher–Ohlin/Stolper–Samuelson models of trade to determine who benefits and who loses under protection and free trade, Rogowski predicts the effects of rising and declining trade on capital-rich and capital-poor economies with different land/labor endowments (Rogowski 1989: 16). Rising and declining trade are seen to have differential effects on relatively abundant and scarce factors, respectively, with different combinations of factors promoting different social cleavages, and from there divergent coalitional outcomes ranging from the U.S. New Deal to Asian fascism.

An important extension of this logic is provided by Hiscox (2002). While Gourevitch (1986) focuses on narrow sector-specific interests and the coalitions they generate, Rogowski (1989) focuses on broader class- and factor-based coalitions; Hiscox wondered if they might both be right. Might it be the case that sometimes we see trade politics played out on a broad class-based canvas and at other times in narrow sectoral politics, and if so, why? Hiscox links both bodies of work together by focusing on the degree of interindustry factor mobility, and in doing so he expands the reach of both theories. His argument is simple and elegant.

If factors are mobile between industries, we can predict broad-based class politics. If they are specific and immobile, we can predict narrow sectoral conflict. In other words, “class conflict is more likely when levels of factor mobility are high . . . industry-based conflict is more likely when levels of mobility are relatively low” (Hiscox 2002: 5). The logic is that in the early stages of industrialization factor mobility is high since skills are basic and transferable, technology is simple, and transportation innovations have large effects. As such, both capitalists and workers have broadly similar, albeit orthogonal, interests. More developed political economies, however, produce “more specific forms of human

⁵ Rather than briefly survey a large number of works in each of these three traditions, as other authors in this volume have done, I have decided to focus on a few critical exemplars in each tradition.

and physical capital and far greater complementarity between technology and labor skills” (Hiscox 2002: 11). This, in turn, implies that workers’ and capitalists’ interests become more cross-cutting, giving rise to new nonclass or factor based coalitions. Using this framework, Hiscox is not only able to show how and why forms of trade politics vary, but also offers insight into important questions of economic history and economic policy.

Assets, Skills, and Compensation

Another significant contribution to this literature that uses a similar “interests and assets” framework is made by scholars who highlight the importance of skills in the political economy. Insofar as the skills held by labor and sought by employers are valuable assets, focusing on skills might shed light on, for example, the highly varied forms that welfare states and labor markets take around the world. Although more obviously linked to the varieties of capitalism literature in political science (Thelen 2004; Hall and Soskice 2001), this literature also owes its existence to the focus on sectors, assets, mobility, and exogenous changes in the world economy that lie at the heart of Gourevitch’s concerns.

Pioneers of this literature are Mares (2003) and Iversen (2005). Mares takes as her point of departure the observation that in explaining the development of social policies, it is usually assumed that workers and employers have orthogonal interests (Korpi 1978; Esping-Andersen 1985). But given employers’ need for skills and labor’s need to acquire them, might it not be possible that Gourevitchian coalitions of interest between different employers and workers across sectors could have played a role here (Mares 2003: 2–9)? Conceiving of skills as mutual investments in specific assets by both employers and workers, Mares is able to view social policies, and indeed entire regimes of social protection, in a new light. Rather than view such outcomes as something “won or lost” by labor in its contest with capital, Mares views social policies as employer-produced devices to encourage workers to invest in skills and thereby insure against specific production risks. Mares’s logic is powerful and compelling.

If the marginal productivity theory of wages is correct, then returns to skill account for the differential in wages observed across an industry. As such, it is reasonable for workers to learn skills and earn more returns to their factor. Skills in this sense become tradable assets. The problem is, of course, that learning skills is a risky business for both workers and employers. From the worker’s point of view, not only is it costly to train, but the more skilled you are, the more redundant (substitutable) one may become if technologies change or terms of trade turn against you (Iversen 2005: 3). Yet, from the employer’s point of view, if you spend a lot of money training workers and they can easily move to a rival firm, why bother doing so in the first place? But you still need skilled workers.

Mares argues that in order to convince workers to share some of the costs of training, the possibility of nonemployment due to their skill set has to be factored into workers’ estimations of their future income streams in making their decision to undertake training (Mares 2003: 24). Social policies are best seen,

then, as a subsidy paid by either the state or the employer (itself a function of the degree of risk an employer faces) to support the reservation wage of skilled workers such that the returns to their skills (assets) do not fall below the market-clearing rate of the unskilled when they are unemployed. For if they did, why would anyone bother to learn a trade? Variations in employment protection can then be understood as employers' responses to trading off heightened production risk against securing adequate skilled labor via mechanisms of internal firm or external state control. Social policies can then, in the aggregate, benefit employers as well as workers, which is why employers help set them up. Meanwhile, the coalitions of workers and capitalists that promote such policies can then be deduced given the nature of assets, risks, and skills in a given economy (Mares 2003: 21–63).

Taking this logic further is Iversen (2005).⁶ Building on Mares's analysis, Iversen argues for a micro-level "asset theory of the welfare state" (2005: 11). Like Hiscox, Iversen seeks to unite and extend two interest-based theories. The first is Mares's foil, the "power-resources" school of welfare state development, which sees labor and labor-backed parties as pushing business into producing welfare policies against their will (Korpi 1978; Esping Andersen 1985). The latter is Mares's own approach, in which employers actually set up the welfare state (Mares 2003). Key in reconciling these positions is to see the former's focus on redistribution and the latter's focus on insurance as two sides of the same coin rather than as opposing positions. Making this move allows Iversen to "understand how popular preferences for social insurance and redistribution are rooted in people's position in the economy, how these preferences are aggregated into social policies, and how these policies in turn affect individual investments into assets that shape economic performance and interests" (Iversen 2005: 13).

Drawing on the varieties of capitalism literature (Hall and Soskice 2001), Iversen posits that economic systems that rely on general skills (typically, the Anglo-American model) create wage inequality and poverty since they "limit . . . incentives for skill acquisition at the low end of the academic ability distribution" (Iversen 2005: 18). Such systems specialize in low-skill manufacturing and increasingly in services, areas in which productivity enhancement through skills is limited. Consequently, few such skills are supplied, and over time the wages of those at the bottom end of the distribution fall. However, despite their position in the labor market, such workers may not prefer higher taxes and more redistribution since "without an investment in distributive insurance, investment in general skills is the best defense against adverse changes in the labor market" (Iversen 2005: 24). In contrast, where the economic system relies upon more specific skills (typically, in continental Europe), training regimes and compensation via insurance come to the fore. Given this alternative set of institutional complementarities, workers demand greater welfare transfers and pay more taxes to shoulder the costs of becoming, in effect, "specific assets." Just as Hiscox unites Rogowski and Gourevitch through a focus on mobility, Iversen unites

⁶ Iversen's piece is an extension of his work with David Soskice. See Iversen and Soskice (2001).

rival views of the origins of the welfare state through a focus on insurance and redistribution as complementary products and as the basis of political coalitions.

Interest-Based Theories: Strengths and Weaknesses

Taken together, this body of literature has several strengths. First of all, it links macro-level changes in the economy to a set of plausible microfoundations grounded in agents' material environments; there are no explanatory "leaps of faith" from one level of analysis to another. Second, such approaches are parsimonious and predictive. Clear and testable propositions can be deduced from them. Third, they put the politics back into political economy by putting the question of distribution front and center. These approaches take seriously the question "Who benefits?" They do, however, have some characteristic limitations, perhaps the most salient of which is how the powerful beam of illumination such theories produce has over time become perhaps somewhat distorting of the historical record.

Take Rogowski's description of mid-nineteenth-century Britain as an alliance of abundant factors (capital and labor) against the scarce factor (land) (Rogowski 1989: 10). While this has intuitive plausibility, it also sidesteps the following question: To what extent, in a predemocratic United Kingdom, where political action by labor was met with imprisonment and exile to the colonies under the Transportation Acts, could a "political coalition" in any meaningful sense have actually existed? Similarly, Hiscox's focus on the effects of factor mobility leads him to impute to the authors of the postwar Swedish model of capitalism, Gösta Rehn and Rudolph Meidner, a desire to promote intraindustry factor mobility as a way of limiting rent-seeking (Hiscox 2002: 6, 162–163). Again, this is a logical consequence of the model, but it is hard to find direct evidence that Rehn and Meidner ever thought this way. In fact, what they did say shows a desire to use mobility to enhance productivity while squeezing profits, which is an entirely different rationale (Meidner 1980; Blyth 2002).

These reinterpretations of history become even more pronounced in the work of Mares and Iversen. Here the works of generations of labor historians, and quite a few political scientists, are thrown into question in what is a rather apolitical reading of events where strikes, lockouts, shootings, revolts, Communist Party agitations, socialist Sunday schools, and hunger marches all become quite puzzling. After all, employers apparently *wanted* to give workers a welfare state, so what was all the trouble about? This is a rather bold position that may risk sacrificing historical accuracy for theoretical fit by reducing violent political struggles where interests were opposed to a coordination game where labor activists and social scientists, each lacking the correct theory, somehow misread business' intentions. Countering such claims, Mares notes that this scholarship argues that "the recognition that employers have been active participants in the creation of the welfare states does not imply that . . . workers have been passive by-standers . . . [n]or does it imply that employers have been agenda-setters in welfare state reform" . . . [because] . . . "it is often difficult to distinguish empirically among [actors]

underlying preferences and strategic motives” (Mares this volume). Yet, if one cannot make such distinctions, can one really link actors’ intentions to outcomes via their material interests, as this literature presumes?

For example, during the 1920s, Belgian capitalists insisted on including transportation subsidies in welfare packages. In this sense, they helped build the welfare state. But the question remains: Why did they do so? Under this framework, one could argue that they did so to solve a coordination problem – getting labor to work – but one could equally argue that they wanted such subsidies so that workers did not live near their factories and therefore could not organize (Liebman 1979).⁷ So, did employers build the welfare state to secure skills or demobilize labor? It is perhaps worth recalling that the labor power thesis that this “skills-based” explanation of welfare institutions is often juxtaposed to is a bit of a straw man. In the original power-resources literature that this school juxtaposes itself to, no one ever called Saltsjöbaden “the historic victory”; it was always “the historic *compromise*” – a moment when capital *compromised* because labor had the whip hand (Blyth 2002; Hacker and Pierson 2002; Korpi 1978). Reducing all politics to a series of coordination problems and incentives might fit the model, but it might also, inadvertently rob interest-based political economy of its politics.⁸

DEVELOPMENTS IN INSTITUTIONAL POLITICAL ECONOMY

Chosen Structures That Structure Choices?

As argued previously, what differentiates an institutional from an interest-based explanation is the contention that *institutions structure choices* and are prior to interests. However, as also noted previously, there is a parallel reading of institutions that views them as *chosen structures* designed to reap gains in trade and/or solve collective action problems (North 1990). In examining extensions of this body of scholarship, what stands out is how later institutionalist work has attempted to synthesize both sides of this literature. This desire to do so is seen most clearly in recent game-theoretic extensions of institutionalist analysis (Hall and Soskice 2001; Greif 2006).

Paralleling work by Iversen et al. (1999), Hall and Soskice (2001) develop a micro-level explanation of the continuing institutional diversity found among the advanced industrial economies despite globalization’s supposed “regression to the institutional mean.” This “varieties of capitalism” approach takes firms to be the primary agents of institutional construction and focuses on their strategic interaction in game-theoretic terms. Firms, seen as actors in a series of infinitely sequenced games, are both the product of and, more importantly, the producer of specific institutions. These institutions come into being and evolve over

⁷ I thank Mimi Keck for this example.

⁸ One could also argue that the model is an “as if” analogical/predictive, rather than an “as is” empirical model, and hence the preceding critique is redundant. But in response, one could counter, why put weight on archival evidence of what employers actually thought and did if the exercise is “as if”? See Friedman (1953) for the relevant distinctions.

time as a function of nationally based firms' attempts to find solutions to five coordination problems that jointly determine the particular variety of capitalism that a country has (Hall and Soskice 2001: 9–21).⁹ Over time, such strategic interactions among firms pursuing their interests produce self-reinforcing institutional complementarities via feedback loops and increasing returns, which in turn produce two distinct regime clusters, Liberal Market Economies (LMEs) and Coordinated Market Economies (CMEs).

In this version of institutional theory, the weighting is very much on firms' choices creating institutions, which then serve as the context of subsequent choices. But note that those institutions are causally powerful only to the extent that these firms find the institutional complementarities created by them to be useful (Blyth 2003b; Howell 2003). In this sense, firms' interests are conditioned by and endogenous to the institutional content, but the origins and maintenance of these institutions are exogenous properties of firm's prior strategic interests. Therefore, while the varieties of capitalism literature represents a progressive extension of institutionalist theory, it does so by cleaving to what is, at base, a less "institutionalist" understanding of institutions.

Also working within a game-theoretic tradition is Greif (2006). Greif, however, uses game theory to move institutionalist political economy in another direction entirely. While viewing institutions as "chosen structures" that "structure choices," for Greif the weighting is the other way around. For Greif, institutions are no mere context or instrument. Rather, "institutions are the engine of history" (Greif 2006: 399). Greif's empirical purpose is to explain the role of institutions in producing the late medieval economic expansion, and to analyze how different institutions led to different outcomes among European and Islamic communities (Greif 2006: 25, 252–255, 300–301).

Greif begins by attacking the chosen structures/structuring choices dichotomy. As he puts it, "[t]hese two seemingly contradictory views on institutions . . . must be bridged because each captures an important feature of reality" (Greif 2006: 41). He aims to do so by rethinking what an institution is and to then embed this in a game-theoretic framework (Greif 2006: 14–28, 35–39). Greif views institutions as "elements" that can be creatively recombined by agents rather than as objects cut from whole cloth. As he puts it, "[t]hese institutional elements are exogenous to each individual whose behavior they influence. They provide individuals with the cognitive, coordinative, normative, and informational micro-foundations . . . [that] . . . motivate them" (Greif 2006: 14).¹⁰ Key

⁹ Those coordination problems are bargaining between business and labor over wages and working conditions, securing suitable skills, gaining access to finance, issues of interfirm cooperation and competition, and adverse selection in employees. Institutions here certainly structure firms' choices, but they are first and foremost the product of those choices. This marks a departure from Hall's earlier work. As Hall and Soskice note, "[h]ere we depart from our own previous formulations as well" (Hall and Soskice 2001: 12, fn 11).

¹⁰ His other definition of institutions stresses the same elements where institutions are "man-made non-physical factors that generate regularities of behavior while being exogenous to each individual whose behavior is influenced" (Greif 2006: 21).

here is the stress on motivation and cognition, or, as he puts it, “cultural beliefs . . . influence the selection of institutions,” since the “motivation provided by beliefs and norms . . . is the linchpin of institutions” (Greif 2006: 28, 45). Viewing institutions as a compound of beliefs and motivations leads Greif to explain variation in the practices of Christian and Muslim traders as a function of their particularistic beliefs and the institutions such beliefs made possible. In brief, individualism and weak kin ties brought about the corporation and growth in Europe; strong kin ties and collectivist beliefs, in contrast, brought about in-group sanctioning, a mistrust of difference, and restricted trade networks in the Arab world (Greif 2006: 25, 253, 300–301, 389–400).

In contrast to Hall and Soskice (2001), where game theory is largely used metaphorically, Greif actually uses game theory, but his employment of it is rather unusual. Greif is well aware of the standard criticisms of game-theoretic models as being too determinate if perfect information is assumed and completely indeterminate if information is less than complete (McKelvey 1976). As such, its relevance for “the games real actors play” is often questioned (Scharpf 1997; Munck 2001). Greif accepts these criticisms and then uses institutions to rehabilitate game theory. Accepting that the assumptions underlying game theory are completely unrealistic, Greif argues that institutions “compress” information into rules and norms that allow agents to act as if they were the fully informed agents of game theory (Greif 2006: 126, 138).¹¹ As such, Greif is able to give a game-theoretic *institutional* account of institutional stability and change, growth and decline, where material interests pale before beliefs and ideas.

Non-Game-Theoretic Extensions: The Developmental State Literature

A final body of institutional scholarship worth examining is the literature on the developmental state in East Asia. This scholarship took an entirely different path from the previously discussed works and developed a form of institutional analysis that is much more classically “historical institutionalist.” That is, this literature is more obviously related to works such as Katzenstein’s *Small States* (1985) and Hall’s *Governing the Economy* (1986) since it deals with institutions as contingent historical products that structure choices, but it does so from the basis of the inductive observation of particular cases.

Deyo’s *The Political Economy of the New Asian Industrialism* (1987) was the first major statement of this approach. Deyo and his contributors sought to explain why East Asian industrialization succeeded while most developing states’ industrialization efforts failed. In explaining this outcome, Deyo et al. highlighted the role of the state in industrial development and, like Hall (1986), stressed the importance of institutional linkages. Specifically, the volume highlighted how institutions

¹¹ Note, Greif does not model “as if” agents were fully informed, but assumes that agents can actually act as if they were fully informed given the informational proxies that are institutions and norms. This turns Friedman’s famous dictum about the realism of assumptions inside out (Friedman 1953).

that promoted savings for investment capital, insulated state bureaucracies, and facilitated credit controls were unique parts of the East Asian experience.

Three contributions built upon this opening: Haggard (1990), Wade (1990), and Amsden (1989). Haggard (1990) examined why Brazil, Mexico, Taiwan, Korea, Hong Kong, and Singapore all began their industrialization drives with a strategy of import substitution industrialization (ISI), yet only the East Asian states managed to change to a more sustainable export-led growth (ELG) strategy. Haggard's answer to this puzzle invokes factors as diverse as egalitarian land reforms and the size of the domestic market, but what really drives his argument is state autonomy and, crucially, the *institutions* that make coherent long-term industrial policy over the heads of the powerful possible (Haggard 1990: 23–48, 261–264).¹² This is why, according to Haggard, despite having similar initial conditions and strategies, East Asia flourished while Latin America stagnated.

While Haggard situated critical domestic institutions within a global context, Wade (1990) and Amsden (1989) grappled with the details of East Asian industrialization to see how rather than “governing the economy,” East Asian states sought instead to “govern the market.” For both of these authors, how the state organized, rationed, and authorized inputs was crucial in explaining their success. At base, rather than “getting the prices right” via wholesale liberalization, these authors show how East Asian states actively sought to get the prices wrong in order to direct capital into specific sectors to overcome the problem of the small size of their domestic markets and move up the value chain via ELG (Wade 1990: 108–112, 157–158, 191–194). Such studies had a clear purpose: to explain how these states succeeded while others failed, with the answer being “the right institutions.” But such an answer simply begged another question: What made those institutions the right institutions?

Key in addressing this issue was Evans (1995), who sought to explain variations in the growth and development of the information technology sectors of Brazil, India, and Korea. Creating two ideal types of predatory and developmental states, both of which are interventionist but only one devolves into chronic rent-seeking, Evans argues that what matters is how the state simultaneously penetrates and yet remains uncaptured by society. Key in doing this is a specific type of bureaucracy, one that is meritocratic and efficient. Evans argues that developmental states with such “Weberian” bureaucracies exhibit a quality he calls “embedded autonomy,” where “highly selective meritocratic recruitment creates . . . corporate coherence” inside the state, which facilitates the degree of autonomy necessary to formulate longer-term developmental projects. However, this autonomy is “embedded in a concrete set of social ties that [allows for] . . . continual negotiation” by societal actors. As he puts it, “only when embeddedness and autonomy are joined can a state become developmental” (Evans 1995: 12). To make this explanation more than tautologous, Evans brings

¹² As Haggard puts it, “a critical test of the institutional perspective is whether state structures can insulate political elites from the demands of the powerful. This is what happened in . . . Korea, Taiwan, and Singapore” (Haggard 1990: 264).

institutions to the fore, arguing that institutions give content to interests since they “have a reality that is prior to ‘individual interests’” (Evans 1996: 28). As such, institutions, historically contingent and hard to replicate, once again appear as the source of growth and development.

Extensions to this literature are voluminous and form a critical part of contemporary political economy approaches to development. Some scholars have moved away from the notion of autonomous and enlightened elites maximizing pigovian welfare functions to stress how mutual hostage taking in different parts of the state explains variations in development (Kang 2002). Others have examined cases of failure and found that even the best Weberian bureaucracies can be thwarted by noncooperation by capitalists (Chibber 2003), while some are indirectly aided by an ideology of isolation (Woo-Cummings 1999). Regardless, this literature has made a significant contribution to our understanding of the complex roles played by states in development.

INSTITUTIONALIST POLITICAL ECONOMY: STRENGTHS AND WEAKNESSES

Like the interest-based approaches discussed earlier, institutional approaches exhibit both considerable strengths and some signature weaknesses. One major strength is that they “move beyond the tendency of conventional economic analysis to treat all developed [and developing] economies as if they were institutionally identical” (Hall 1997: 182). In doing so, institutionalist accounts give us some leverage on why similarly placed agents in similar structural positions do not in fact act the same way across cases. They are also a reminder of how complex political economies are and how thinking institutionally allows theorists to compare across cases. What we also see in this literature is a useful “coming together” of both sides of the institutional chosen structures/structured choices dichotomy. Scholars such as Hall and Soskice have become far more attuned to how institutions are chosen structures, while scholars on the other side of the divide, such as Greif, are increasingly concerned with how they structure choices. There are also, of course, some general limitations to institutionalist analysis, such as the availability of comparative cases, but as each branch of this literature has developed, each exhibits its own characteristic strengths and weaknesses.

The approach to institutionalism exemplified by the “varieties” approach of Hall and Soskice has quickly become one of the most copied, and critiqued, positions in the literature. Criticisms of this type of institutionalist political economy tend to revolve around three issues. First, there is the tendency to confuse ideal types with real existing political economies, with the result that the more one takes the ideal to be real, the fewer actual cases the model seems to fit (Crouch 2005; Campbell and Pedersen 2007). Second is the tendency, similar to that found in Mares and Iversen, to see political struggle as a “too-rational” process where functional fit and self-regulating equilibrium institutions replace more basic political struggles (Howell 2003). Third is the attribution of causes to sets of institutions whose purported effects may be generated elsewhere (Blyth 2003b).

Greif's institutionalism, emblematic of the recent "sociological turn" of economics (Greif 2006: 22), is burdened by a separate set of concerns. Perhaps the most basic concern is whether the game (theory) is worth the candle. While multimethod interdisciplinary work is to be applauded, such explanations must add value to other approaches. Subtract the game theory exposition of the cases and the reinterpretation of everyday economic and sociological concepts, and one is arguably left with a rather strong culturalist framework where, because agents think in certain categories, given the institutions that their unique ideas and beliefs allow them to build, such agents act in specific ways, given the institutions that their unique ideas and beliefs allow them to build (Seabrooke 2006: 26). Why one needs higher math to reach a rather tautological conclusion that anthropologists grew dissatisfied with a generation ago is an open question.

Finally, the analyses that grew out of the focus on the developmental state also have several limitations. In contrast to our other two versions of institutionalist political economy where "theoretical overreach" may be a problem, what this literature exhibits is arguably a *lack* of theoretical reach. Being inductively generated from the examination of a few select cases sets up a classic "survivorship bias" problem (Taleb 2007).¹³ By correlating the attributes of "winners" as causes, when "losers" are encountered they are examined from the premises generated by analyses of the successful cases, thus biasing the results. Given this, the portability of the framework outside of the cases where it was developed, a strength of deductively derived interest-based approaches, is severely attenuated, as recent discussions of the relevance of the developmental state model for the Chinese experience demonstrate (Tsai 2002, 2007).

BEYOND COALITIONS AND INSTITUTIONS: IDEATIONAL POLITICAL ECONOMY

As argued previously, Katzenstein's *Small States* (1985) opened the door to moving beyond material and institutional approaches, suggesting that ideas and ideologies needed to be taken seriously as explanatory concepts in their own right. This call has been taken up by a diverse group of political economists. While such approaches began with, and to some extent retained, a focus on how elites instrumentally use ideas as resources, more recent literature has sought to go beyond this instrumentalist position. To trace how this branch of political economy has evolved over time, I group ideational works under two headings: ideas as resources and conventions, and ideas as governance technologies.¹⁴

¹³ I prefer this term to the more usual "selection on the dependent variable" term since it more accurately specifies the problem.

¹⁴ I use the terms "ideational" and "constructivist" interchangeably. Although constructivism has its home in international relations and ideational scholarship has its home in comparative politics, they ultimately investigate similar phenomena. While the first generation of such scholars used different categories and concepts – ideas versus norms, for example – what is of interest is how both schools increasingly come together and, in doing so, dissolve what is left of the barrier that separates international and comparative political economy. See Hobson and Seabrooke (2007).

Ideas as Resources and Conventions

An important early statement of this approach was Peter Hall's study of the spread of Keynesian ideas across nations (Hall 1989). Building on his prior work on how institutions structure choices, Hall examined different national receptions of Keynesian ideas as a way of explaining institutional change. Specifically, how individual actors used Keynesian ideas as resources to forge new social coalitions during the dislocation of the 1930s comes to the fore in this analysis. In contrast to materialist models, in Hall's view, exogenous shocks to agents' material positions do not unproblematically translate into new political preferences without some kind of elite mediation as to what such shocks signify; hence the importance of Keynesian ideas. For Hall, what determines the degree to which such elite mediation is possible, and whether such ideas can recast political debate and thus possible lines of cleavage, is the ideas' degree of "fit" with the existing "structure of political discourse" of a nation (Hall 1989: 383).

Building on Hall's opening were several contributions that created a head of steam for ideational scholarship during the 1990s. Notable and influential were Sikkink's examination of how developmentalist ideas impacted economic policy-making in Latin America (Sikkink 1991); how ideas can serve as "road maps" and focal points in situations of multiple equilibria (Goldstein and Keohane 1993); and how neoliberal ideas informed the project of EMU (McNamara 1998). What these contributions all have in common is the *instrumental* use of ideas. That is, ideas are used by agents to realize their goals. However, within this literature there is, implicitly at least, another position: that ideas are not reducible to agents' *a priori* material interests or institutional position (McNamara 1998: 6–9). Once "let out of the box," ideas "have a life of their own" and can take interests in new and unexpected directions. To understand this subtle but important distinction, consider the work of Berman (2006), Blyth (2002), and Jabko (2006).

Berman asks two questions: First, why, during the crisis of the 1920s and 1930s, did two sets of political ideas come to prominence: social democracy and fascism? And second, why was the set of fascist ideas so successful in its time? Berman focuses squarely on ideas and how elites use ideas, not just to fashion coalitions, as stated earlier, but in a prior step, to interpret what changes in the political economy *mean for other agents* (Berman 2006). In doing so, Berman moves beyond ideas as individual resources and stresses the importance of ideas as intersubjective conventions that can restructure agents' interests.

Berman argues that fascism came to power because of the "cognitive locking" Marxist thinking encouraged among left parties in the 1920s and 1930s. Marxism was, especially for the German Social Democrats (SPD), the paradigm through which contemporary developments were understood. As such, rigid adherence to the axioms of historical materialism prevented the SPD from reacting in any positive way to the Depression. After all, if capitalism was going to come tumbling down, and Marxism told you why, and also told you that this was a good thing, why try to stop it? Trapped within this logic, left parties became forces for political inaction, which paradoxically laid the groundwork for the rise of

fascist parties. Unencumbered by such ideas, fascist parties were able to offer alternative diagnoses of and hence alternative policies to the Depression that differed from those of liberals (“just wait until things get better”) or those of Marxists (“just wait; it can only get worse”). Fascist parties were then able, through argument and action, to create a new narrative of what the Depression meant (and who was responsible for it) that recast agents’ interests and served as the basis of their new coalitions. For Berman, ideas are indeed instruments, but they are instruments that can recast interests. They are social technologies that frame and decode complex environments such that intersubjective understandings can be created among differentially located actors as a necessary step in mobilizing collective action.

My own work very much follows in this tradition of viewing ideas as both resources and conventions. In *Great Transformations* (Blyth 2002) I offered an explanation of why one common phenomenon, deflation, produced institutional responses as varied as Japanese imperialism, Italian fascism, and Swedish social democracy. Focusing on ideas as interfaces with the world constructed by agents to make sense of complexity and uncertainty, I sought to show how economic ideas formed the basis of both the distinctive social coalitions and new institutions that emerged in these periods in the United States and Sweden. I argued that ideas matter for political economy explanations to the extent that phenomena such as deflation and inflation can produce nonprobabilistic Knightian uncertainty, a situation where agents can be unsure of what their interests are because following their first-best strategies leads to Pareto-inferior outcomes (Blyth 2002, 2007, 2008). In such moments, when individually rational actions lead to collectively suboptimal outcomes, uncertainty over what has gone wrong and what to do about it leads to an indeterminacy between structure and action where, as Ira Katznelson has put it, “structurally induced unsettled times can provoke possibilities for particularly consequential purposive action” (Katznelson 2003: 274). In these unsettled times, the power of ideas *to make agents powerful* comes to the fore.

This theme is developed further in the work of Jabko (2006). Jabko asks where the ideas for the institutional design of the European Union (EU) came from. For Jabko, a focus on French elites (Parsons 2003) or on a transnational “neoliberal consensus” (McNamara 1998) is necessary but insufficient to explain the institutional form of the EU. Jabko focuses instead on the role of the European Commission, a weak actor at the heart of the EU as the generator and transmitter of some very constituency-specific ideas (Jabko 2006: 42–57). As a weak actor that nonetheless held a critical position in the emerging European system of transnational governance, Jabko argues, the Commission “worked at constructing a particular integrationist agenda . . . premised on the renewed popularity of market ideas” (Jabko 2006: 48). The Commission thus “sold” the project of integration to different constituencies according to logics those constituencies wanted to hear.¹⁵

¹⁵ To telecommunications and finance firms the drive for market integration was sold as a constraint to be overcome. To monopolistic power generators the market was appealed to as a norm to aspire to. To other weaker EU members, the market was sold as a common space of interaction

By “playing the market” in this way, the Commission was able to bring other, more powerful actors on board and create momentum for a series of institutional reforms that were in no way reducible to the supposed material interests of the actors in whose name these reforms were made, including the Commission itself.

In all of these accounts, what comes across clearly is the contingency of political change and institutional development. Rather than the linear equation of “structural position → interests → actions” that we find in interest-based accounts or the path dependence of institutional accounts (Pierson 2004; Mahoney 2000), these approaches stress, as do constructivist accounts in general, how things “could have been different” had it not been for certain socially constructed (ideational) factors (Hacking 1999; Hay 1999). Given this, what other perspectives take as almost overdetermined, these accounts portray as underdetermined and dependent upon the particular constructions wielded by less than powerful actors. Once again, ideas are not simply weapons wielded by powerful agents, although they can be that. Instead, ideas are contingent properties that can make agents powerful (Epstein 2008).

Ideas as Governance Technologies

A final contribution to this literature worth mentioning goes further still. These authors seek to understand how particular ideas not only serve as resources or intersubjective conventions, but are themselves technologies of governance. One such attempt of note is the work of Paul Langley, who applies actor-network theory to the study of pension privatization and mortgage securitization (Langley 2004, 2006a, 2006b, 2008). Again, while materialist and institutionalist analyses of these phenomena do point to important determinants of choice and action (Brooks 2005), what Langley brings to our attention is how such “top-down” projects rely upon not just an appeal to agents’ interests, but an attempt to craft particular subjectivities and subject positions.¹⁶

Langley analyzes how pensions in the United States and the United Kingdom have increasingly moved from the province of the state to the market, characterized by a decline in state-based funding and a general shift from defined benefit to defined contribution schemes (Langley 2006a: 920). This much is well known. What is not so obvious is how such schemes rest upon creating a particular subject, what Langley calls the “entrepreneurial investor subject” (Langley 2006a: 921). Crucially, such subjects are not simply “out there” as already constituted individuals waiting around for the financial services industry to pick them up.¹⁷ After all, why should, for example, a technical worker in an

and a level playing field. Finally, the project of EMU as a whole was sold as a talisman of “all good things going together” to the EU member states as a whole (Jabko 2006: 57–179).

¹⁶ On the limits of “top-down” approaches where elites deploy ideas and masses passively accept them, see Seabrooke (2006).

¹⁷ Indeed, as the British pensions debacle of the 1990s showed all too well, without massive governmental intervention to promote the individualization of pensions, financial firms want to have nothing to do with such small investors (Langley 2004).

export industry have a given “interest” in a defined contribution program? Such subjects must be discursively constructed before they can act in the political economy as entrepreneurial agents.

For Langley, the creation of such subjects rests upon “the sidelining of insurance as a means of . . . managing . . . risks in favor of the promotion of investment . . . to bear risk.” In such a world, risk is no longer “represented as potential dangers to be collectively managed . . . [instead] ‘risk’ [is] represented as . . . a reward for individuals” (Langley 2006a: 921). Creating such subject positions is then not simply a matter of appealing to agents’ pre-given interests. Rather, it is a process of discursive construction where instruments as varied as popular television programs on investment, state-sponsored savings programs (401[k]s), and calculative technologies of credit reporting are brought to bear to create particular risk-bearing subjects who “perform” risk-bearing practices.

Taking this focus on “performativity” further, that is, investigating how the action of employing ideas that seek to represent or measure a given phenomenon brings the phenomenon into being, is MacKenzie’s work on the performative role of financial theory in the economy (MacKenzie 2006). For MacKenzie, financial ideas do not merely describe the world; they can also help bring that world into being. As he puts it, “financial economics . . . did more than analyze markets; it altered them. It was an ‘engine’ . . . an active force transforming its environment, not a camera passively recording it” (MacKenzie 2006: 12).

For MacKenzie, economic analyses do not stand outside of reality, “analyzing it as an external thing” (MacKenzie 2006: 16). Rather, as is common in complex social systems, there is an *interdependence* of subject and object such that beliefs about the former influence the behavior of the latter. As a consequence, applying an idea changes the nature of the system where it is applied (Blyth 2008). This is more than Merton’s famous idea of a “self-fulfilling prophecy” (Merton 1968). In this case, by performing theory, MacKenzie draws attention to how the use of specific models in financial markets becomes part of the infrastructure of markets. That is, such ideas constitute the “algorithms, procedures, routines, and material devices” that *are* markets (MacKenzie 2006: 19).

Given this object/subject interdependence, MacKenzie shows how the use of particular financial technologies, their “performance” over time in the world, made the world more like the theory (MacKenzie 2006: 143–179). In particular, the use of the Black–Scholes options pricing model by market participants, which made assumptions about markets having zero transactions costs and being efficient, helped legitimate arguments and policies designed to make markets more efficient (MacKenzie 2003: 854). Doing so, in turn, enabled agents to employ the model in ever wider areas of finance such that the fit between the model and the reality it purported to describe increased over time. As he puts it, using the model “altered patterns of pricing in a way that increased the validity of the model’s predictions” (MacKenzie 2003: 852).

In a wonderful case of causal recursion, it was not that the model’s fit got better over time as its accuracy grew. Rather, the model’s usage by more and more actors made the markets themselves behave more like the model

(MacKenzie 2006: 263–268; 2003: 852–856). These developments, in turn, enabled agents to deploy ever more sophisticated risk-calculating technologies based on these same ideas that, in turn, allowed the development of the “risk society” and risk-bearing subjects Langley is concerned with (MacKenzie 2006: 211–243). In sum, these approaches show how ideas not only inform but *constitute* structures of governance. Following Hirschman (1978), Langley and MacKenzie show how ideas themselves can constitute and alter the world they supposedly describe. By constructing subjects and performing theory, we gain insight into how governance is not simply a property of government, but is something that occurs on and through individual agents.

Ideational Political Economy: Strengths and Weaknesses

Like interest-based and institutional approaches, ideational approaches have specific strengths and particular weaknesses. One strength is that such work enables analysts to ask questions that other approaches do not even consider asking. After all, if one takes complex mediated phenomena such as the causal generators behind deflation, globalization, and multigenerational institution building projects to be equally obvious to all agents, then there would be no point in attending to ideas. However, as these examples show, while interest-based accounts can point to coalitions that “should be there,” given sectoral alignments, and while institutional accounts can point to path dependencies in policymaking, ideational approaches drop down below the level of the possible to investigate what real actors thought and did. As such, the problematic “reimagining of history” noted earlier is avoided. Second, that agents’ subjectivities and interests can be reconstructed despite their ostensible structural positions, as this literature also demonstrates, is ruled out of bounds by other approaches. In this literature, interests are treated as something to be explained rather than something to do the explaining with. Third, this literature also excels in explaining change. Its focus on the contingent and dynamic nature of political and economic change offers a very different vantage to the soft functionalism of interest-based and institutionalist approaches that sometimes see the world as all too stable. However, like our other approaches, it has certain problems.

First of all, disentangling the effects of ideas from other factors, if seen as possible at all, is extremely difficult. That agents thought in a certain way may be established, but the extent to which outcomes are directly attributable to these ideas is still difficult to ascertain. Strategies can be applied to attack this problem (Parsons 2003, 2007), but the “How much do ideas matter?” question never really goes away.¹⁸ A second perennial question is “Where do such ideas come from?” This is a problem because if ideas are reducible to agents’

¹⁸ Unless one sees interests as indistinguishable from “ideas about interests,” in which case the problem disappears completely (Wendt 1999; Widmaier 2004; Blyth 2003a). This is a step few American political economists are willing to take. In contrast, such a position is arguably the dominant one in other parts of the world. See Blyth (2009) for a discussion of these issues.

interests, and if they really are just instruments to help people get what they want, then the ideational story collapses into an interest-based account.¹⁹ Finally, there is the quite reasonable question “What do such approaches do to our ability as social scientists to generalize and predict?” After all, if contingency, construction, and interdependence effects are as replete as at least some of these scholars say they are, then whether political economy can aspire to the status of a predictive social science is questionable at best.²⁰ Given these concerns, while ideas may matter for political economy explanations, it is not clear that they need matter for all the questions that political economists ask.

The Link Not Made? Rational Choice Theory and Political Economy

I return now to the first caveat we opened with: the relationship of political economy to rational choice theory. After all, one could tell several other versions of this story that include rational choice as a core component of political economy. One version would be a more expansive set of four concepts rather than three: rationality, interests, ideas, and institutions, for example. This version of events would have Arrow (1948) and Von Neumann and Morgenstern (1953) laying the conceptual foundations upon which Downs (1957), Riker (1962), and Olson (1965) build the basic concerns of the field: spatial models, bargaining, collective action, strategic interaction, to name but a few. Following this, one could point to Bates on development (1981), Elster on technical change (1983), Przeworski on left parties and social democracy (1986), and North on institutions (1990) as the “killer apps” of the period, and then, again as Levi has done in her contribution to this volume, map recent developments from there. Indeed, there would be nothing wrong with doing so, except for the fact that it would essentially repeat Levi’s chapter. Since she has done such a good job of this, I see no point in repeating her effort here. Apart from redundancy, however, there are other good reasons for not including rational choice theory, and rationalism as a concept, as its own category in *this* discussion of political economy: namely, that interests as a concept are not exhausted by rationality as a behavioral postulate, and that political economy is broader than rational choice and vice versa.

The first issue is best exemplified by going back to the extensions of interest-based political economy discussed earlier. One could argue, quite reasonably, that while Iversen, Mares, Hiscox, et al. can trace their substantive ideas to Gourevitch, their thinking, both theoretical and methodological, is rationalist. As such, they are more “rational choice theorists” than “interest-based” political economists. I would not even be surprised if the aforementioned

¹⁹ The response, that ideas are emergent properties of complex adaptive systems, irreducible to the conditions of their emergence, worries many political economists since it introduces a very large slice of indeterminacy to our explanations.

²⁰ That political science can ever be a predictive science given the nature of the social world is tackled in Blyth (2006).

scholars would self-identify in exactly this way. My point here in mapping the field in this way is somewhat different. Even though this chapter seeks to draw boundaries using discrete concepts, if one makes interests a derivative function of rationalism, then one misses how other schools of political economy also deal with interests: in terms of how our interests are endogenous to the institutional context or from analyzing our “ideas about our interests.” That is, theories of institutions are also theories of interest generation, and theories of ideas are also theories of institutional change. By reducing “acting on one’s interests” to “acting according to a rationalist behavioral postulate,” one risks ignoring what large numbers of scholars who also regard themselves as political economists have to say about these same questions.

Second, political economy is broader than rational choice theory, and rational choice theory is broader than political economy. For some rational choice theorists, rational choice theory is political economy – period. Consequently, if you are not doing what they do, then you are not a political economist. Consider, for example, the introduction to the *Oxford Handbook of Political Economy* (2006). According to the editors of this *Handbook*, “[I]n our view, political economy is the methodology of economics applied to the analysis of political behavior and institutions” (Weingast and Wittman 2006: 3). Political economy is a field where “the unit of study is typically the individual. The individual is motivated to achieve goals . . . the theory is based in mathematics . . . [and] . . . sophisticated statistical techniques” (ibid.: 4). Moreover, political economy is defined by its common foci: “endogenous institutions,” “the revelation and aggregation of information,” and “the concepts of survival and equilibrium” (ibid.).

Now, while I fully admit that this is *an* approach to political economy, one that can happily sit within the interest-based approaches outlined here, it also defines itself in relation to sets of concepts and categories that effectively says to many of the other scholars discussed here that whatever it is they are doing, *it is not political economy*. Rather than adjudicate the “one true version” of the field, I find it more useful to point out here that political economy may be seen as a broader field with more concerns than are often portrayed by some rational choice theorists. Indeed, as Professor Levi notes in her chapter, far from being monolithic, rational choice theory “now tends to be multimethod and catholic in its approach” such that rather than squeezing out other approaches, “the competitiveness I described between the camps [rationalists and historicists] in the earlier edition of this volume has subsided” (Levi this volume). In sum, I argue that rather than rational choice positioning itself *as* political economy, it is more usefully seen as an extremely important general position in comparative politics that has evolved into far more than an application of the methodology of economics to any and all political economy questions as defined solely by the concepts of equilibrium, endogenous institutions, and the like. That this is the case is clearly seen in the embrace of ideational and other variables by rational choice theorists, not as a takeover bid, but as a broadening of the perspective itself.

CONCLUSIONS: COMPARING APPROACHES AND THE QUESTION OF BOUNDARIES

Having said all that, I do not want the reader to leave this survey with the idea that the field started with interests, moved to institutions, and now embraces ideas as some kind of teleological progression. This is simply not the case. All three of these positions are vibrant research programs. That they emerged at the same time out of the core concerns of three exemplar works is significant, but one did not succeed the other. What I wish to return to here is the issue of the boundaries of political economy and our second opening caveat: the extent to which this diversity of approaches is something to be concerned about or embraced. Key in determining this is the extent to which these different approaches speak to one another. To the extent that they do, political economy as a field has a certain coherence. To the degree that they do not, it remains simply a collection of approaches within comparative politics that happen to focus on economic questions.

As we have seen, each strand of political economy has a distinctive evolution. Interest-based political economy has, when seen in terms of the overall history of political economy, had the most peculiar evolution. We noted earlier how political economy was reborn in the 1970s as a partial alternative to formal economic analysis. In its canonization in *Gourevitch (1986)* this strand of political economy put interests front and center, but saw those interests as nonetheless mediated by a host of secondary factors. As noted, however, later contributions have been far more reductionist. This strand of the literature has increasingly become more like mainstream economics with an emphasis on equilibrium modeling and formal theory, which is somewhat ironic when one remembers that what “kick-started” this approach was a rejection of economics’ separation of politics and markets and its attendant reductionism. This strand of the literature now seems to be repeating this separation insofar as politics in these approaches is increasingly reduced to a series of coordination games and/or equilibrium states to be modeled. For some scholars this is a concern; for others, it is a sign of progress.

Indeed, the evolution of one strand of institutional political economy in part reflects this “back to economics” tendency, while other parts of it have headed off in entirely different directions. As seen in the approaches reviewed here, theoretical divergence and convergence coexist in this literature. Varieties-type approaches explicitly set as a goal a desire to bring the literatures of economics and political science together in a game-theoretic synthesis (*Hall and Soskice 2001*). But approaches are not defined simply by particular technologies. Greif’s use of game theory, for example, productively steps beyond its usual materialist/rationalist formulations insofar as it is embedded in a cultural and sociological approach that rejects notions of fixed interests and institutions as products of conscious design. As such, this type of institutional political economy can deploy economic technologies without pushing political economy back into economics. Finally, there is more mainstream historical institutionalist political economy,

here identified with, but not limited to, work on the developmental state. This body of work has continued the type of scholarship that Hall (1986) exemplified, and has become perhaps the most common form of institutional political economy practiced in the field.

Ideational political economy is the most variegated type and is also the most recent vintage. As such, it is harder to give an account of its evolution. What one can say is that it has become an approach to be reckoned with in its own right. Although this approach contains within it everyone from “thin” constructivists to actor-network theorists, it has established itself as more than just a helper for other approaches (c.f. Goldstein and Keohane 1993) or as appropriate only for peculiar “limit cases” where normal dynamics break down. Its diversity of approaches, and the types of questions it asks, certainly add to the breadth of political economy approaches, and do so in a way that, even more than institutional works, pulls away from a “reeconomizing” of political *economy* toward a genuine postdisciplinary stance.

In sum, if one does not reduce political economy to a singular approach, it appears as a diverse and thriving area of study. Given that the object of study of political economy contains everything from microphenomena to large-scale social processes, such diversity is probably, on balance, a good thing. After all, in an uncertain world, diversification is generally regarded as a prudent strategy – not just to protect us from the “all the eggs in one basket” problem, but to position us to take advantage of the unexpected events that often destroy singular theories (Taleb 2007; Blyth 2006).

The Global Context of Comparative Politics

Etel Solingen

The literature on the international sources of domestic politics has an important pedigree in comparative politics in the works of Herz (1950), Gerschenkron (1962), Hintze (1975), Gourevitch (1978, 1986), Cameron (1978), Skocpol (1979, 1992), Bates (1981), Tilly (1985), Katzenstein (1985), Putnam (1988), Rogowski (1989), and world system and dependency theory, among others. Although, as Gourevitch (1996: 8) argued over a decade ago, “comparative politics have never been wholly autonomous from international influences,” the end of the Cold War marked an explosion in the study of internationalization, globalization, and their effects on domestic politics.¹ This chapter addresses developments in this literature published since this volume’s first edition.

The relationship between domestic and international politics entails two kinds of linkages (see Figure 9.1). The first are known as “second-image reversed” effects (Gourevitch 1978), referring to the impact of international forces on domestic politics, from the outside-in. Many among the intellectual forerunners in the study of domestic–international linkages cited earlier fall under this category. The second kind – “second-image” effects (Waltz 1954) – link domestic structures and institutions to international politics from the inside-out. These two types of effects intersect in the domestic realm, where a process of conversion takes place. States, political parties, social movements,

¹ Prominent among these was Rosenau (1997). Internationalization involves the expansion of global markets, institutions, and norms, a process progressively reducing the purely domestic aspects of politics (Solingen 2001). More aspects of domestic politics are exposed to external phenomena, but domestic responses are not uniform (Keohane and Milner 1996a). Reduced barriers to international flows of goods, capital, and ideas do not necessarily imply global convergence (Gourevitch 2003). “Globalization” is a far more ambiguous term, defined in different ways by the different authors reviewed in this chapter.

I acknowledge very helpful guidance from Mark Lichbach, Alan Zuckerman, and other contributors, particularly those who attended the Bellagio meeting, and the Social Science Research Council–MacArthur Foundation for a Fellowship on International Peace and Security that launched me into comparisons between the Middle East and East Asia discussed in the second half of this chapter. I also thank Cecelia Lynch, Miles Kahler, Bob Keohane, Margaret Levi, Herman M. Schwartz, and Art Stein for very useful comments and Colin Moore, Maryam Komaie, and Wilfred Wan for their research assistance.

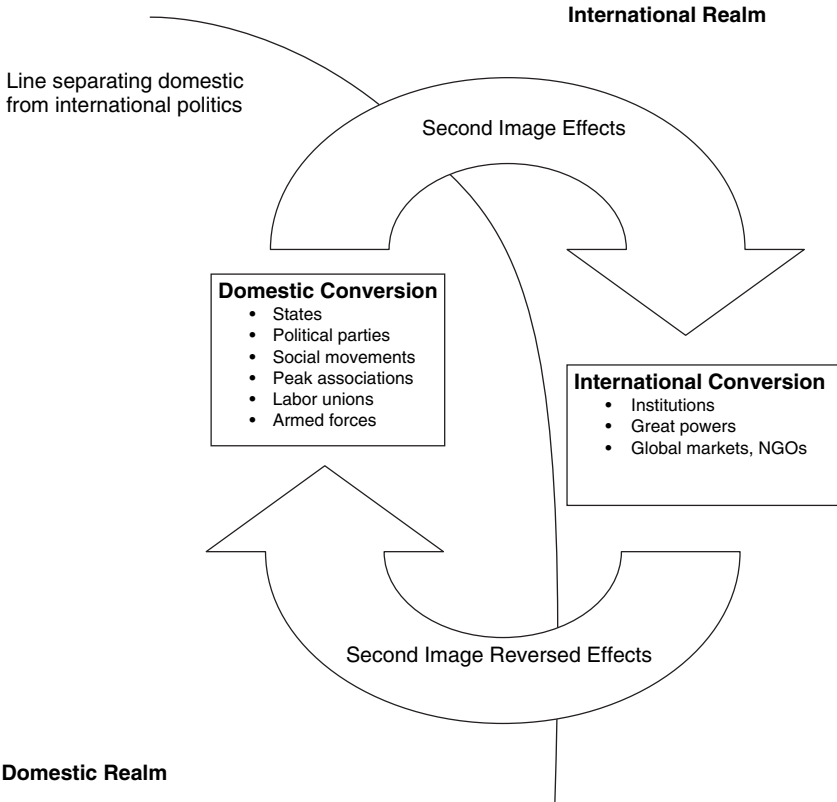


FIGURE 9.1. Types of domestic–international linkages

peak associations, labor unions, policy networks, armed forces, and other collective and individual actors respond to “second-image reversed” effects – on the basis of rational incentives and normative tendencies – with preferences and policies that feed into “second-image” effects from the inside out. Both types of effects also intersect in the international realm, where another process of conversion takes place. International institutions, global markets, great powers, nongovernmental organizations (NGOs), intergovernmental organizations (IGOs), multinational corporations (MNCs), and other collective and individual actors (the United Nations secretary general, the pope) respond to outside-in effects – on the basis of rational incentives and normative tendencies – with preferences and policies that feed into inside-out effects, as conduits from international to domestic politics.

Given the nature of this volume, this chapter dwells largely on “second-image reversed” effects and domestic conversion. Internationalization/globalization in the past decade has made the international sources of domestic politics a *sine qua non* feature in the study of comparative politics. The context of comparative politics is now more global than ever before, in small and large states alike (Katznelson and Shefter 2002). Indeed, some have gone as far

as proposing that diffusional perspectives on the impact of globalization on domestic policies suggest that “the whole logic of politics may have shifted from a domestic to an international orientation in the past decade” (Jahn 2006: 403).² Students of social movements observe that internationalization appears to be leading to an ultimate fusion between domestic and international activism (Tarrow 2005). This evolution has amplified Galton’s problem severalfold, changing the field of comparative politics as we knew it.³ “Second-image reversed” effects are, fundamentally, mechanisms of global diffusion; taking them into account constitutes a progressive move in the study of domestic politics. A sample survey of the literature published in the past decade suggests strong interest in outside-in effects.⁴ Inside-out effects have been the long-standing subject matter of studies on the domestic sources of foreign policy and international relations (Rosenau 1969) and are less comprehensively addressed here, as are processes of international conversion.⁵ Domestic conversion cannot be isolated from either type of effects.⁶

How does this literature fare in the debate over structure, culture, and rationality as analytical choices? To a significant extent, the sampling of work published after 1997 appears to conform to Lichbach’s (this volume) claim that “paradigm warriors and theoretical fundamentalists are shunned; paradigm busters and synthesizers are honored.” To be sure, studies holding on to paradigms as foundational heuristic devices have far from disappeared. But many also rely on complementary paradigms, suggesting high hybridism and mutually profitable intellectual exchange. More studies rely predominantly on structure, rationality, or both, but culture – alone or in conjunction with others – has gained increased attention. Structure and rationality can be seen as one and the same paradigm; the assumption of rationality allows one to generate predictions from knowledge of a structure.⁷ Similarly, many studies focus on the structural aspects of culture. As Ross (this volume) argues, structure can be seen as both reflective of and derived from culture. Nonetheless, to retain this volume’s terminological consistency, the first section of this chapter, “Structure,

² Jahn acknowledges that an analysis postulating the primacy of international diffusion is unable to specify functional impacts that only domestic analysis can capture.

³ Galton’s problem points to the pitfalls of assuming that single entities (a state’s domestic politics, for instance) are independent of each other rather than the result of either a common source or borrowing that diffuses across the population of entities under study (Naroll 1964).

⁴ This informal survey included books published by major university presses and articles in leading journals. I acknowledge Colin Moore’s assistance with this survey.

⁵ Work primarily addressing international conversion or the influence of international variables on each other, though extraneous to our undertaking here, is a burgeoning subject in international relations.

⁶ Easton’s (1965) “political system” largely contained within states is evolving into a truly global one where the external and internal environments become more inextricable.

⁷ Keohane (private communication). According to Gourevitch (1978: 904), “structure affects the extent to which a governing coalition must make sidepayments to build up its strength, the extent to which it can impose its views.” Fearon and Wendt (2002) regard structure as a theoretical viewpoint, whereas culture and rationality are considered analytical tools, metaphysical positions, or empirical descriptions of the world.

Rationality, and Culture in Domestic–International Linkages,” overviews representative work along the three paradigms, noting the high incidence of hybridity. The second section, “Domestic–International Linkages in East Asia and the Middle East,” puts this literature to work in the context of a broad cross-regional comparison between East Asia and the Middle East as a hands-on exercise on the links between comparative and international politics. It examines theories and causal mechanisms cast in structural, cultural, and rationality terms – as well as hybrids – that might guide further efforts to understand these regions’ dramatically different domestic and international trajectories in recent decades. The comparison thus serves three purposes: It pulls some of the relevant theoretical literature together in the analysis of a core issue (the divergent evolution of the two regions); it heeds Lichbach’s call for engaging big questions and puzzles and Zuckerman’s plea for seeking theory-informed explanatory mechanisms; and it does so by addressing a critical – hitherto largely overlooked – comparison clamoring for deeper understanding.

STRUCTURE, RATIONALITY, AND CULTURE IN DOMESTIC–INTERNATIONAL LINKAGES

Like any other classification, the one guiding this volume can force studies into analytical straightjackets, but it also retains utility for taking stock of a vast literature. Space constraints preclude the chapter from reflecting all nuances in the study of domestic–international linkages, but this section’s overview provides a window into the kind of paradigmatic warfare and reconciliation that constitute this volume’s leitmotif. Each subsection begins with a brief synthesis of work within a paradigm, offering a flavor of general themes and findings through selected representative work.

Structure

Internationalization has rendered “second-image reversed” effects a focal interest and a modal form in the tradition of structural analysis. Pioneer work in this area includes the vast literature in dependency theory, particularly in the 1960s and 1970s, as well as the work of Diaz-Alejandro (1981), who found that few generalizations could be drawn about the link between openness to the global economy and authoritarianism. The more recent literature in the past decade examines the impact of international structure (international power distribution, global markets, international institutions and norms, and their agents) on domestic structures (states, political parties, party systems, policy networks, civil society, social movements, shared norms) as well as on processes and individuals. A significant proportion of studies addresses the impact of economic globalization (MNCs, technology, trade, capital mobility, development aid) on macroeconomic policies; corporate governance; social expenditure; wages, income distribution, and social democracy; domestic regulation, government spending and revenue generation; sectoral industrial change; and

democratization, among others.⁸ Quite often, studies examine the relative weight of international versus domestic structures. Kastner and Rector (2003), for instance, examine both international structural (financial) forces and the number of domestic veto-player parties in government to explain capital control policy. They find that the independent effects of domestic structures have disappeared since the 1980s, when systemic constraints increased substantially. Others explore the very nature of links connecting the domestic and global economies as causal variables. For instance, civil war is variably traced to heavy reliance on oil exports (de Soysa 2002; Fearon and Laitin 2003) and primary commodities exports (Doyle and Sambanis 2000) but not to legal agricultural commodities exports (Ross 2004).

Beyond economic actors, other studies address the effects of external threats and conflict on democratization; of UN interventions on domestic stability, peace, and development; of regional organizations on transitions to democracy; of “transboundary formations” on political authority in Africa, and of transnational conflict on civil wars, particularly in regions with low levels of democracy and trade.⁹ A significant share of constructivist work on second-image reversed effects defies the structure–culture demarcation. Such studies explore, for instance, the impact of international social structures of meaning and social value on states’ perceptions, preferences, and actions; and how global advocacy networks influence domestic social movements in human rights, the environment, and women.¹⁰ Other instances of hybridity are discussed later.

Many studies along structural lines cast to examine the impact of globalization find instead that domestic political factors are far more resilient than is sometimes hypothesized. Domestic structures were found to continue to shape egalitarian income distribution, social transfers, public expenditures, liberalization of international capital flows and technology choices.¹¹ Crepaz and Moser (2004) examine how different domestic electoral systems respond to globalization differently, increasing or decreasing social expenditures. Globalization may constrain state behavior or enable state capacities, contingent on the character of domestic institutions (Weiss et al. 2003). As Mosley (2003) suggests, international financial integration does not end social democratic welfare policies or entail policy convergence among advanced democracies, but it influences industrializing states’ policy autonomy more markedly without eliminating choice. Work in this vein largely avoids simplistic international or domestic structural reductionism, effectively seeking to uncover how domestic and international structures interact or scope conditions under which one might be more dominant than the other. Kaufman and Segura-Ubiero (2001)

⁸ Iversen (1999); Boix (2000); Gourevitch (2003); Rodrik (1997); Schwartz (1998); Birdsall et al. (1999); Kapstein (2000); Rudra (2002); Bardhan et al. (2006); Jahn (2006); Thatcher (2004); Remmer (2004); Moore (2002); Acemoglu and Robinson (2006).

⁹ Colaresi and Thompson (2003); Doyle and Sambanis (2000, 2006); Caplan (2005); Pevehouse (2002); Brown (1996); Callaghy, Kassimir, and Latham (2001); Sambanis (2001); Gleditsch (2007).

¹⁰ Finnemore (1996); Keck and Sikkink (1998).

¹¹ Iversen (1999); Mahler (2004); Brooks (2004); Solingen (1996b).

found that trade integration has a consistently negative effect on aggregate social spending, compounded by openness to capital markets. Popularly based governments appear more likely to protect social security transfers while decreasing spending on health and education; shifts toward democracy expand such expenditures.

There is a voluminous literature involving second-image reversed studies on the sources, nature, and future of the state itself as the primary political structure.¹² These studies address the impact of international structures (economic, military, political, normative) on state forms. An early precursor was Herz (1957), who predicted that nuclear weapons might end the “hard shell” of territorial states as we knew them. Half a century later, nuclear weapons have arguably had much less of a role in state transformation than have other variables. Herz (1976) later acknowledged the fragility of new states, drawing attention to the international sources of intervention to “harden” them, a visionary insight given an increasing international role in buttressing failed states (Herbst 2000). Another precursor, Hintze (1975), traced Prussia’s “strong state” to its geopolitical predicament, softening that position after World War I. Important counterpoints to structuralism included Eckart Kehr (1973), who depicted the domestic “marriage of iron and rye” that gave life to the great German battle fleet – *Der Primat der Innenpolitik* – while wielding “foreign threats” as a rationalization. I return to this theme later. A more recent structural theory (Waltz 1979) treated states as undifferentiated “like units” arranged according to structural or relative power and struggling for survival in an anarchic international system with no legitimate authority. This approach made the internal character of states, leaders, and culture extraneous for understanding international relations. There have been vigorous conceptual and empirical rebuttals of this theory within the past decade.¹³ Indeed, structural power so conceived has been found deficient even in arenas that provide the easiest tests for such theories, such as nuclear behavior. Only adequate understanding of domestic politics can explain why – facing similar structural conditions – states respond variably, some by acquiring nuclear weapons and others by renouncing them either unilaterally or through international institutions (Solingen 1994, 2007a).

Rejecting rigid structural theories of power, Rosecrance (1999) argues that globalization is replacing the old foundations of state power – territorial size and stockpiles of classical factors of production – with new ones, embedded in the flows of goods, capital and labor as well as managerial, financial, and creative capabilities.¹⁴ These changes are arguably turning states such as Singapore, Hong Kong, Taiwan, and Switzerland into “virtual states” with

¹² Because Migdal’s chapter reviews the literature on the state, I cover it only briefly here. On the state of “the state” from an international relations standpoint, see Kahler (2002). On how global war shaped global powers, see Rasler and Thompson (1989).

¹³ Vasquez (1998); Milner (1997, 1998); Legro and Moravcsik (1999); Walker and Morton (2005).

¹⁴ On international determinants of state size, see Alesina and Spolaore (2005).

diminished home production; others into intermediate states focused on services; and yet others into manufacturing, all three linked in productive partnerships. Rosecrance finds this process bound to turn military conquest into a nonsensical idea (an inside-out effect). Whereas territorial states aimed at conquest, trading and virtual states pursue international commerce and mastery of ideas, knowledge, creativity, capital, and information. The domestic consequences of states' virtualization (outside-in effects) should not be unemployment if states do what they must: provide improved and lifelong education and retooling, develop effective regulatory capacity, maintain macroeconomic stability, strengthen the middle class, attract foreign investment, and protect property rights – but also remain small, humble, and limited in power without becoming too weak relative to market forces. This is a tall order, one that studies cited in this chapter address piecemeal. The virtualization of states requires a theory of domestic politics that explains why some states evolve in one direction or another. States are pivots linking internationalization and domestic politics and often arbiters of that relationship. They can be both barriers and conduits to internationalization, compelling the need to explore the political, bureaucratic, and moral agents pushing in one direction or another, their rational incentives, the cultural and institutional contexts that shape their preferences, and the conditions under which they are likely to achieve their preferred outcomes. I return to these vital questions in the section on the comparative politics of East Asia and the Middle East.

An important body of work on state sovereignty has emerged in the past decade. Philpott (2001) explores the international sources of sovereign states and finds them in two global revolutions of ideas: the Protestant Reformation leading to Westphalia and norms of equality and nationalism that ended colonial empires circa 1960. One meaning of sovereignty is the state's absolute authority within its borders and the absence of any higher authority beyond its borders (Reus-Smit 1999). Both constructivist and rationalist literatures recognize that the theoretical premise of sovereignty as the basic structuring principle of international society is strongly challenged in practice. Krasner (1999, discussed in the next section) argues that the autonomy of domestic structures has always been compromised through external coercion, imposition, contracts, or conventions. Globalization may lubricate mechanisms of intervention, and perhaps create new ones, but it follows much older historical paths. States have been far more resilient than some of the literature on globalization has stipulated. Nonetheless, studies along the *long durée* explore potential alternatives or competitors to the state, some of which have or could acquire trappings of sovereignty, from cities to regional institutions, export production zones, and empires.¹⁵

Lastly, “second-image” effects emphasize the impact of domestic political structures – states, regime types – on international structures, a subject I expand upon in the next section.

¹⁵ Spruyt (1994); Sassen (2001); Kahler (2002).

Rationality

Many studies advancing the rationality paradigm explore the incentives of maximizing domestic and international strategically interactive agents regarding decisions or processes that transcend the domestic–international divide. Domestic actors are both affected by outside-in effects from the international environment and respond to it through inside-out effects flowing back into the international arena. With increased internationalization – certainly not a new phenomenon – political leaders find themselves progressively more enmeshed in the interstices between both types of effects. For instance, several studies focus on leaders facing two-level games vis-à-vis domestic and international audiences.¹⁶ Studies along these lines have sought to explain leaders' incentives to push for domestic transparency to signal their own credibility to domestic and international constituencies; how leaders use international actors to frame identity and mobilize support for favored policies vis-à-vis communal conflict; why leaders select international legal dispute resolution over bilateral negotiations; how leaders enact tax reforms while competing for international capital; and how leaders weigh domestic audience costs for making empty threats, arising from constituents' concern with their leader's or country's international reputation.¹⁷

Other studies examine states' and private actors' incentives, interests, and preferences regarding regional trade arrangements, international environmental regimes, European monetary reform, international bank regulation, international cartels, international organizations, bargaining arrangements with labor, trade openness and public sector size, European Union (EU) legal mechanisms and membership conditionality, and preferential trade agreements as enforcers of human rights.¹⁸ Some studies find that globalization contracts welfare spending (Burgoon 2001); others that it has a weak influence (Huber and Stephens 2001) or arguably expands it (Swank 2002). Much of this literature relates primarily to affluent Organization for Economic Cooperation and Development (OECD) states and employs formal and quantitative methods. Many offer nuanced interpretations of the impact of globalization on domestic politics. Pierson's (2001) analysis of welfare states rejects hyperstructuralist traps, pointing to the crucial role of domestic institutions. Similarly, Hicks and Zorn (2005) point to complex effects of globalization on welfare retrenchment via different causal mechanisms, some of which produce "paradoxes of self-limiting moderation." Trade openness and financial liberalization are found to inhibit welfare retrenchments, whereas outward-oriented foreign direct investment

¹⁶ Putnam (1988); Evans, Jacobson, and Putnam (1993); Frieden and Martin (2002).

¹⁷ Grigorescu (2003); Grove (2001); Allee and Huth (2006); Basinger and Hallerberg (2004); Tomz (2007).

¹⁸ Chase (2003); DeSombre (2000); Walsh (2001); Oatley and Nabors (1998); Bates (1997); Moravcsik (1998); Goldstein, Kahler, Keohane, and Slaughter (2000); Goldstein and Martin (2000); Broz and Hawes (2006); Mansfield and Pevehouse (2006); Thelen and Van Wijnbergen (2003); Adserà and Boix (2002); Alter (2000); Kelley (2004); Hafner-Burton (2005).

creates pressures to contract them. Kahler and Lake (2003) conclude that globalization changes the policy preferences of some actors, increases the bargaining power of others, and opens new institutional options for yet others. As the last three volumes suggest, second-image reversed effects are far more contingent than Manichean versions of globalization would have it.¹⁹

Studies leaning on rational choice also generally provide a more flexible understanding of state sovereignty than purely structural ones. Against a common view that globalization threatens the continued viability of sovereign states, Krasner (1999) argues that rulers have often violated sovereignty norms to stay in power – by invitation or by outside coercion and imposition. Westphalian sovereignty – states’ right to exclude external authorities from their own territory – has thus been mere “organized hypocrisy.” Some rulers sacrifice state autonomy to external powers or international institutions to satisfy domestic or external constituencies, weaken domestic or external competitors, tie successors’ hands, or strengthen values they cherish. Others sacrifice no autonomy to external powers or institutions for precisely the same reasons, with alternative constituencies and values in mind. Krasner’s (1999) view thus differs from either material hyperstructuralist or normative interpretations of sovereignty, emphasizing rulers’ calculations of material and ideational interests, not taken-for-granted practices derived from “overarching institutional structures or deeply embedded generative grammars” (9). Coercion and forceful intervention can pose particular threats for weaker and “failed” states, an important outside-in effect examined by Finnemore (2004).

Studies in the rationalistic tradition have also explored the relationship between leaders’ political survival – their efforts to gain or remain in power – and their calculus regarding war and peace, an angle I explore in greater detail through the comparison between East Asia and the Middle East later in this chapter. An important subset of this literature examines diversionary mechanisms, or how leaders turn to external scapegoats, strengthen in-group solidarity by singling out a “threatening” out-group, and “gamble for resurrection” through war to avoid losing power (Goemans 2000). Bueno de Mesquita, Smith, Siverson, and Morrow (2003) find that leaders who rely on small winning coalitions (such as autocrats) have greater incentives to engage in wars with territorial ambitions than those requiring large winning coalitions (such as those elected democratically). The former tend to pursue private goods in war; the latter public goods. Nondemocracies are more prone to gamble for resurrection through war; to lengthen wars and tolerate higher war costs; and to survive in power longer than their democratic counterparts. Chiozza and Goemans (2004) also find nondemocratic leaders at high risk of losing office to be least likely to become targets of external challenges. Democracies are found to be more prone to avoid wars among themselves but not with nondemocracies; more likely to choose their wars wisely and win them at lower costs; prevail in wars they do

¹⁹ On global-domestic interactions in international political economy, see Frieden and Martin (2002).

fight; be more reliable allies or less reliable ones; abide by their international commitments; credibly demonstrate resolve but be more selective about making threats; and enter into more preferential trading arrangements than their counterparts.²⁰ Gowa (1999) finds the “peace among democracies” to be an artifact of Cold War alliances rather than domestic norms or institutions.

Others focus on the external implications of the transition to democracy. Mansfield and Snyder (2005) find the early stages of transition, with weak democratic political institutions and politicians’ incentives to resort to violent nationalism, to be conducive to war. The logic of unintended consequences operates here as well: war becomes the inadvertent by-product of belligerent provocations, a phenomenon to which I return later. Mansfield and Pevehouse (2006) find that leaders presiding over democratic transitions tie their hands by joining international organizations (IOs), particularly democratic “clubs.” This commits leaders to democratic reform by increasing the cost of ex-post policy changes and signals democratic credibility to external actors. Democratic transitions thus encourage IO membership independently of democracies’ tendency to join democratic clubs. Much of the literature linking democracy to outside-in or inside-out effects blends institutional and microrational considerations (Solingen 1996a). Related work moves even further away from purely paradigmatic thinking by considering not only how democratic incentives but also norms and culture influence – and are influenced by – international variables (Doyle 1986; Owen 1997).

Culture

As argued earlier, cultural studies of second-image reversed effects sometimes are hard to differentiate from structural arguments. For instance, some constructivist studies impute international normative structures with shaping domestic processes, although, given their epistemology, they advance far more nuanced claims about causality.²¹ Finnemore’s (1996) landmark study traced states’ purpose and behavior less to external threats or domestic political considerations than to networks of transnational and social relations that shape states’ perceptions of the world. Others trace the influence of international law on liberal ideas; of international norms on human rights standards; of liberal democracy on civil war; and of EU norms on Eastern European politics.²² To be sure, international norms do not diffuse automatically or consistently across

²⁰ Russett and O’Neal (2001); Reiter and Stam (2002); Lake (2003); Leeds (2003); Gartzke and Gleditsch (2004); Fearon (1994); Gaubatz (1996); Schultz (2001); Mansfield, Milner, and Rosendorff (2002).

²¹ Constructivism is not a substantive but a social theory that makes claims about the nature of social life and social change. It does not make particular claims about the content of social structures or the nature of agents and hence does not, by itself, produce specific predictions about political outcomes that can be tested (Finnemore and Sikkink 2001; Klotz and Lynch 2007).

²² Reus-Smit (2004); Sikkink, Risse, and Ropp (1999); Foot (2000); Thomas (2001); Peceny and Stanley (2001); Schimmelfennig and Sedelmeier (2005).

states, compelling process tracing of mechanisms of norm diffusion into domestic environments (Gurowitz 1999). Pauly and Reich's (1997) influential article challenged the logic that globally integrating markets – mobile capital and the need for MNCs to respond to cross-national technological and financial incentives – ultimately drive corporate behavior. Instead of isomorphic MNCs, they find no convergence toward core common patterns of behavior but rather that national institutional, ideological, and historical legacies shape internal governance, long-term financing, research and development activities, investment and trading strategies, and other corporate decisions. This historical-institutionalist tack aligns culture and structure while relying on a wide range of empirical evidence, from aggregate and sectoral statistical data to case studies and personal interviews.

Cultural studies of inside-out effects find, among many others, that xenophobia, not merely economic interests, explains opposition to EU integration (De Master and Le Roy 2000), and that economic nationalism, not merely statism or protectionism, influences approaches to regional integration (Abdelal 2001).

Hybrids (“Paradigm Busters”)

As the discussion thus far suggests, many arguments on domestic–international linkages are irreducible to a single paradigm and are effectively nuanced exercises in trespassing paradigmatic trenches. This trend conforms to Lichbach's formulation that “in the eclectic, sprawling, and internally diverse literatures that characterize the field, drawing hard-and-fast boundaries between frameworks is fruitless. Rationality, culture and structure are porous” (Lichbach 1997a). Studies connecting structure and rationality recognize that rational actors operate in a bounded structural or institutional universe (Milner 1998). Institutions and structures limit choices among strategically interactive actors, but choice still exists. Studies exploring outside-in effects examine how globalization elicits various forms of social resistance, both constraining and creating opportunities for contentious transnational movements; how international financial actors and domestic governments interact strategically; how trade and capital mobility constrain government policy without undermining the viability of social welfare programs; and how local, resource-related factors – rather than globalization – play decisive roles in fueling domestic conflicts.²³ Others find that international trade through European integration undermines support for Italy's Christian Democrats; that trade decreases competition among developed countries' parties; and that right parties are more likely to support free trade than left parties, but greater integration in the world economy reduces partisan competition in developed countries, making all parties more favorable to open economies.²⁴

²³ Tarrow (2005); McAdam, Tarrow, and Tilly (this volume); Leblang (2002); Garrett (1998); Sadowski (1998).

²⁴ Golden (2004); Hiscox (2002); Milner and Judkins (2004).

Doner, Ritchie, and Slater's (2005) study of the origins of Northeast Asian developmental states exemplify the structural-rationalistic blend in the comparative-historical tradition. I return to this study later. Zweig (2002) puts issues of structure and agency succinctly – did China open to the world or did the world open China? – finding the answer both in structural opportunities generated by global markets and in rules created by central elites enhancing local entrepreneurship that helped dismantle barriers to internationalization. In a study of regional orders in economics and security (Solingen 1998), I examined both global structural constraints and incentives and different responses by domestic ruling coalitions. I focused on economic liberalization as the key fault line in the analysis of domestic coalitional realignments in the late twentieth century, against the common background of a rapidly integrating global economy and a multilateral institutional foundation in world politics, unprecedented capital mobility, the growing web of regional interactions and institutions, and a disintegrating revival of ethnic, religious, and nationalist allegiances. Different domestic coalitions were found to formulate alternative grand strategies toward global political and economic structures, and toward the internal extraction and allocation of resources. This conception of domestic coalitional grand strategy transcends the disciplinary divide between economic strategies – the subject matter of comparative and international political economy – and security strategies, as well as the common analytic cleavage between advanced and industrializing states. Politicians everywhere lean on competing domestic constituencies favoring and opposing internationalization, and logroll different coalitions advancing rival grand strategies with synergies across economics and security and across domestic, regional, and global arenas. Policies derived from preferences and resulting grand strategies further influence domestic coalitional cleavages and carry outside-in effects back into the external arena, as captured in Figure 9.2.

Lake and Powell (1999) offer another important synthesis of structure and rationality, making the strategic interaction of actors itself – rather than individuals, states, or policies – the unit of analysis. While their “strategic choice” approach has a distinguished pedigree (Herz 1950; Schelling 1960; Stein 1999), their volume provides coherence to sometime disparate research agendas across comparative and international politics, advancing our understanding of linkages between the two through conceptual clarification of (1) actors' preferences and beliefs over outcomes, separate from strategies and strategic contexts (Frieden); (2) institutional effects on interactions, preference aggregation, and the strategic environment (Gourevitch, Rogowski); (3) specification of the strategic environment – (inter)actions, information – within which actors anticipate what others will do (Morrow); and (4) insight into evolutionary transformations of strategic settings (Kahler). Purposive actors here span the level of aggregation (decision makers, firms, interest groups, classes, political parties, bureaucracies, the state). The analytical move allowing the “strategic choice” approach to separate actors from their environments also separates it from constructivist work for whom actors are constituted by social, cultural, or normative contexts.

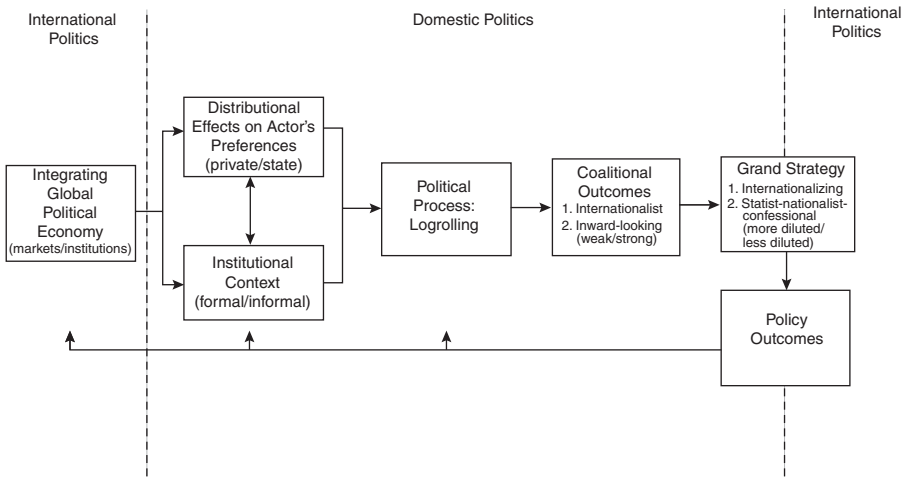


FIGURE 9.2. The global political economy, coalitional effects, and grand strategy. (Reproduced from the original in Solingen 1998: 21.)

As argued earlier, studies bridging structure and culture have also grown in importance. For instance, Nasr (2000) traces domestic identity mobilization both to regional power alignments and to primordial cleavages. Campbell and Pedersen (2001) transcend paradigmatic debates and methodological approaches by identifying complementarities and cross-fertilization among historical/structural, discursive, rational choice, and organizational analysis (the “second movement” in the study of institutions) in explaining the rise of neoliberalism. They find that exogenous shocks (stagflation, debt crisis, unemployment, international competition) trigger changes in institutional arrangements; institutions play mediating roles in spreading neoliberalism; high contestation, bargaining, and contingencies characterize this process; there is little widespread convergence toward common neoliberal institutions; and states can block, adapt, mediate, and reverse neoliberalism.

DOMESTIC–INTERNATIONAL LINKAGES AND THE COMPARATIVE POLITICS OF EAST ASIA AND THE MIDDLE EAST

The preceding overview illustrates the enormous breadth of publications linking comparative and international politics in the past decade. This section puts this literature to work, following Lichbach’s call to identify big problems and significant puzzles, providing a concrete application of some themes identified in the broader literature to particular issues of broad comparative interest. Throughout the twentieth century, the big questions in the discipline addressed the challenges of fascism and authoritarianism. The 1989 demise of the Soviet bloc triggered more focused concerns with the diffusion of markets and political liberalism. Whereas some (Fukuyama 1989) regarded liberalism as emerging victorious out of the struggles of the twentieth century, others (Huntington 1993) presaged a “clash of civilizations.” As the preceding section suggests, the

past decade led to focused preoccupation with this Polanyiesque double movement, diffusing markets and democracy while creating the seeds of countermovements by antiglobalization forces (Barber 1996; Solingen 1998). This tension – while manifested in the domestic politics of virtually every state – is particularly emblematic of the Middle East, which many consider ground zero of the countermovement. By contrast, East Asia seems poised to become ground zero of a dynamic twenty-first-century global political economy.

Why is this dramatic contrast particularly important, fruitful, or interesting? First, at the dawn of the new century, Middle Eastern countries face persistent authoritarianism, economic stagnation and collapse, widespread violence (largely internal but also interstate), human rights violations, vast gender inequities, political radicalism, high illiteracy and unemployment, demographic youth bulges, and rampant terrorism (Arab Human Development Report 2002).²⁵ This state of affairs, in and of itself, we can classify as a Big Problem and a Real-World Timely Question (Lichbach, this volume; Weber 1946). Furthermore, in sharp contrast, most of East Asia has undergone dramatic economic growth; significant reductions in poverty, illiteracy, and unemployment; major transitions to democracy; and noteworthy declines in inter- and intrastate violence (Gill and Kharas 2007). By any measure, this contrast across regions elicits not only what Lichbach (this volume) defines as a “true puzzle” (riddle, paradox, enigma, or tragedy) but a particularly thorny one that remains “unexplained and mysterious . . . intractable, baffling, and perplexing.” Finally, these differences between a cluster of stagnation and malaise in the Middle East and a cluster of dynamism in East Asia are often taken for granted, obscuring the true extent of the puzzle.

Second, despite widespread recognition of these sharp contrasts, one finds very few systematic comparisons between the two regions in either comparative politics or international relations, even though Middle Eastern problems have become ever more acute and evident, while East Asia’s resilience has become ever more apparent following the 1997–1998 financial crises. This regional pair remained largely beyond comparative scrutiny, failing to bring vast scholarship in the comparative and international politics of each region together. Indeed, studies of the Middle East and East Asia often stressed respective unique features, particularly evident in the proliferating cultural understandings of each. Undeniably, each region has unique contextual variables that helped shape it, and “contextualized comparisons” of different cases within each region enable important gauges of distinctive regional properties. At the same time, excessive concentration on regional specificity helps obscure useful cross-regional comparisons, which, as Huber (2003) suggested, have been a rather neglected practice in comparative politics.²⁶ Focused comparisons across regions could help circumvent some of the fallacies of regional

²⁵ Oil price rises have fueled economic growth among some states recently – with second-order effects on neighboring countries – but manufacturing-driven long-term growth has largely eluded much of the region (Solingen 2007b).

²⁶ Systematic cross-regional comparisons entail the identification of regional patterns (Huber 2003), not merely occasional comparisons of one or two cases from each region, although the latter can be useful in other respects if they are attentive to case-selection procedures.

“exceptionalism” afflicting comparative and international relations scholarship in both cases, particularly on the Middle East.²⁷ The tendency toward exceptionalism makes the dearth of systematic comparisons across these two regions less baffling.

Third, the dramatic cross-regional differences observed today would have been enough to raise various puzzles, yet the two regions’ comparable conditions circa the mid-twentieth century deepen the enigma further. Around that time, both regions shared colonialism and postcolonialism as formative experiences, state-building challenges, economic crises, low per capita gross national products (GNPs), heavy-handed authoritarianism, ethnic clashes and civil wars, some cultural similarities (regarding family, gender, and education), low intra- and extraregional economic interdependence, and weak or nonexistent regional cooperative institutions. Indeed, despite notorious difficulties in relying on Mill’s method of difference, one could reflect on how, against those early cross-regional similarities, some crucial explanatory variables might account for the subsequent progressive differentiation and discrepant outcomes observed today. Given certain similarities in important initial conditions as of the 1950s and 1960s, why did the domestic and international trajectories of states across these regions differ so dramatically subsequently?

These differences are deeply rooted in the nexus of outside-in and inside-out effects as they developed over the second half of the twentieth century in each case. In particular, states in both regions differed on forms and degree of insertion in the global political economy. My point of departure (Lichbach’s “Big Concept Research Agenda”) is the fundamental proposition that, at critical junctures throughout this period, political leaders and ruling coalitions embraced different models of political survival in each case.²⁸ *The model in East Asia revolved around economic performance and growth, which entailed an emphasis on export-led manufacturing and promotion of private entrepreneurship. By contrast, the reigning model of political survival in the Middle East revolved around nationalism, inward-looking self-sufficiency, and state and military entrepreneurship, buttressed when possible by oil rents. Once adopted, both models became self-reinforcing, albeit less than deterministic.*

Different structural, rational, and cultural hypotheses (mechanisms, institutions, and middle-range causal arguments) can be conjured up to explain whence these disparate models of political survival may have come from and their implications. I shall limit the possibilities here to selected explanations rooted in outside-in and inside-out effects – the chapter’s theme – gleaned from wider theoretical possibilities.²⁹ Before plunging into candidate

²⁷ Bellin (2004); Halliday (2005); Anderson (2006); Gran, Vitalis, and Binder (1998).

²⁸ A coalitional analysis (Gourevitch 1978) pivoted on different models of political survival (Buono de Mesquita 2003) seems a fitting point of departure for this substantive exploration of second-image and second-image reversed effects. The coalitional and institutional bases of these models are discussed in Solingen (1998). On “critical junctures,” see Levi (this volume); Collier and Collier (1991); and Pierson (2004).

²⁹ This section borrows from Solingen (2007b) but differs in scope (confined here to second-image and second-image reversed effects) and objectives (stimulating discussion of paradigmatic and

explanations, the obvious must be confirmed. Important differences exist within each region. Yet, asking big questions compels us to distill broad important features and rely on ideal types or heuristic devices that transcend historical or “true” realities, and hence are not applicable to all cases equally or indeed to any particular case wholesale.³⁰ Neither model characterizes the universe of cases in each region, but each captures broadly representative modal forms against which real cases can be compared. Some East Asian states retained selective import-substitution until the 1980s but to a much lesser extent than Middle Eastern ones, which monopolized markets (Owen and Pamuk 1999: 131). While clearly not akin to *laissez-faire*, most East Asian models became market-friendly, emphasizing performance in international markets as yardsticks for success.³¹ The contrast with Middle East patterns is evident (Hakimian 2001; Halliday 2005).

These models had their origins in critical junctures (1950s–1960s), beginning in East Asia with postwar Japan, followed by crucial shifts in Taiwan and South Korea; and in the Middle East arguably with the Free Officers’ 1952 revolution in Egypt and analogous transformations throughout that region. These defining years of institutional inception and change carried enormous implications for the two regions’ subsequent, and diverging, socioeconomic and political evolution as well as for their internal and external stability.³² These were indeed years infused with “extraordinary moments of transformation,” “origins of paths,” and fateful “great branching” junctures (Katznelson this volume). The ensuing five decades provide fertile ground for exploring path-dependent processes and self-reinforcing feedbacks, issues of timing and sequencing (Pierson and Skocpol 2002), and mechanisms of change and transformation; strategic interaction among domestic and foreign – individual and collective – actors; and the long-term evolution of intersubjective understandings. I begin with theories, hypotheses, and mechanisms explaining antecedent, permissive, or catalytic conditions that *gave rise* to competing models of political survival (as dependent variables). The subsequent section introduces theories, hypotheses, and mechanisms explaining the *implications* of different models (as independent variables).

The Sources of Competing Models of Political Survival

Structural Sources. Models of political survival are endogenous, by definition, in structural frameworks. But what structures are they endogenous to?

hybrid alternatives explaining models of political survival rather than advancing particular arguments).

³⁰ Weber (1949: 93); Ruggie (1998: 31–32); Eckstein (1975).

³¹ Haggard (2004); MacIntyre and Naughton (2005); Noland and Pack (2005). North Korea and Burma remain important exceptions to modal East Asian trends.

³² Institutions associated with export-oriented strategies often include new investment statutes, property rights, a single exchange rate, and reductions in trade, capital controls, and bureaucratic barriers.

Candidate structural theories that may explain competing models run the gamut from hard, deterministic formulations to more nuanced, problematized, and complex mechanisms. The former are sometimes cast as part of grand designs by the capitalist West to keep “subordinate systems” in check, underdeveloped, and ripe for imperialist exploitation (inter alia Wallerstein 1974). Although such views are sometimes self-described as Marxist, others (Brenner 1977) interpret Marx as anticipating the exact opposite: The logic of capitalist development is deemed progressive insofar as it transforms the “periphery” into something that mirrors the “core,” more industrialized world. The reality of early-twenty-first-century East Asia certainly aligns itself better with this latter view than with the former. East Asia’s transformation – yielding much more equitable income distributions than predicted by dependency theory – dealt a severe blow to rigid, deterministic structuralisms. Some repackaged structural theories turned older arguments on their head, now imputing Western aid with “causing” the so-called East Asian miracle. The premise that the West, by definition, dampens the rise of competitors was particularly weakened as Japan and other East Asian (and European) states grew stronger with Western assistance. Even China’s rise could hardly be conceived without Western markets, capital, technology, and active promotion (Zweig 2002). Furthermore, if the West “executed” East Asia’s transformation, to put it starkly, why would it not do likewise in the Middle East?³³ Can the same structural (imperialist) sources explain dramatically different outcomes in each region? Perhaps, but not without theoretical heavy lifting from alternative outside-in and inside-out effects that can only be uncovered through careful and controlled comparisons of domestic conditions. The same is true for other structural theories dwelling more on cultural than material dimensions of domination, to which I shall return.

We cannot dwell extensively on these broader important structuralist debates here, except insofar as they can be explicitly linked to the emergence of competing models of political survival, our core analytical category. One such possibility, drawn from theories of imperialism, might surmise that Cold War structures accounted for coercive implantation of export-led models in East Asia (under the U.S. aegis) and inward-looking ones in the Middle East (under the Soviet aegis). Both superpowers had incentives to discourage states in their sphere of influence from embracing their rival’s models. U.S. alliances indeed exposed East Asian partners to threats from Cold War dynamics (Stubbs 2005), but so did partnerships with Saudi Arabia, other Gulf states, Jordan, Lebanon, Israel, pre-1969 Libya, and others, all with varying results (Lustick 1997).

³³ Leading Middle Eastern scholar Fred Halliday (2005: 10) warns against “regional narcissism,” or the belief that “the whole world spends all its time plotting and worrying about the Middle East, and that everything that happens in the region is somehow dissimilar to that which takes place elsewhere, and is singularly evil or angelic, depending on the point of view.” Even as he recognizes external pressures, Halliday (2005: 69–70) bemoans the pervasiveness of conspiracy theories in Middle Eastern accounts, which, in his view, lead to overstating external determination and understating regional actors’ significant autonomy.

Several considerations warn against the fallacy of reducing models of political survival to U.S. intentions (which can be repeatedly foiled, as the Iraq war reminds us). Not all states under the U.S. sphere of influence embraced export-led models. Most arguably did not for several decades, even in Latin America, under the tightest U.S. political control. Moreover, regimes subordinated to U.S. designs, such as Rhee Syngman's, resisted export-led models even with South Korea under effective U.S. occupation. Rhee's successor, Park Chung Hee, did not subscribe to the new model upon assuming power (1961), embracing it only in 1963 (and backtracking in the 1970s despite U.S. and World Bank opposition). Chiang Kai-shek and the Kuomintang (KMT) implanted import substitution in the early years, replacing it only later with the export-driven model that propelled Taiwan into a developed economy, a model strongly favored by powerful domestic actors (Nordhaug 1998). Furthermore, the United States failed to implant favored export-led models in Vietnam and the Philippines (1960s), even though the two were the largest recipients of U.S. aid on a per-capita basis.³⁴ Lastly, U.S. and other Western pressures to encourage export-led industrialization in the Middle East failed to yield that outcome in too many instances. The limits of Western power were particularly evident in this area (Halliday 2005: 71), infusing the West with a sense of powerlessness vis-à-vis this region, in 2007 as in 1900. Indeed, the flip side of the hegemonic hypothesis was the ability of "subordinate" states to extract very high strategic rents from their respective superpowers during the Cold War. Middle Eastern states were not coerced into adopting inward-looking models by Soviet hegemony. Rather, inward-looking praetorian revolutions drove leaders toward policies attuned to Soviet models, as Nasser's chief advisor, Mohammed Hasnayn Heikal (1978), documented. As Halliday (2005: 286) claims, "if this was a master-client relationship, it was not clear which one was the master."

More nuanced structural arguments might trace preferences for contrasting models to different international market and institutional opportunities and constraints in the 1960s, the critical years of model consolidation in each region. In the 1960s, however, investment capital was plentiful, Euromoney easy to borrow, interest rates relatively low, trade barriers against industrializing world manufactures fewer, and competition from other labor-intensive states quite low (Koo 1987: 169; Chan 1990). Furthermore, trade grew faster during the 1960s–1970s, and market-based incentives for export-led strategies were thus strong. International institutions (the International Monetary Fund [IMF], the World Bank) and other donors promoted state intervention at the time, but not suppression of private capital. Even subsequently, the effects of international economic institutions were certainly not determinative but rather a constant against which industrializing states responded variably, contingent on the preferences of ruling coalitions in different states.³⁵ The possibility that different choices were made under fairly comparable (permissive) conditions undermines

³⁴ US AID Greenbook <http://qesdb.cdie.org/gbk>. See also Campos and Root (1996).

³⁵ Kahler (1989); Haggard and Kaufman (1995); Al-Sayyid (2001: 168–169, 171).

structural arguments of this sort, pushing the explanation into a more nuanced understanding of domestic–international linkages.

Yet another structural hypothesis traces different models of political survival to available oil resources yielding international rents. Where available, natural resources enabled ruling coalitions to protect themselves against weak performance in other sectors and to provide patronage to beneficiaries of import substitution. However, oil revenues were far less copious in the crucial 1960s – when Middle Eastern models became more entrenched – than they would be after 1974. Still, they arguably provided a cushion not available to East Asian leaders just as import-substitution exhaustion provided them with few options. Stagnation, slow growth, and unevenly distributed benefits from growth increased poverty, left intractable problems of nation-building unresolved, and contributed to foreign aid fatigue (Rothstein 1977, 1988). Dipping balance-of-payments deficits and accelerating inflation in East Asia constrained choices, favoring export-led models (Chan 1990). By contrast, Middle East ruling coalitions were slower in recognizing the end of the brief, “easy” period of economic expansion under import-substitution and continued to spend heavily, leading to inflation, balance-of-payments crises, and further decline (Hirschman 1968). Those protected by oil revenues or remittances responded to the political-economic crisis of the 1950s–1960s by “deepening” inward-looking models rather than replacing them.

This analysis sounds plausible as far as it goes, particularly when overlaying it on arguments about “Dutch disease” stipulating that resource abundance reduced economic growth, eliminating export competitiveness in other goods (Krugman 1987; Weinthal and Luong 2006). From this standpoint, it is unsurprising that ruling coalitions in oil-scarce Morocco, Tunisia, Turkey, and Israel were eventually (1980s) able to promote alternative models. However, other resource-scarce states resisted export-oriented shifts as well, in part because of regional diffusion (outside-in) effects discussed later. Furthermore, Dutch disease may be less determinative than is sometimes assumed. Some Gulf monarchies began diversifying away from oil dependence, forging a new relationship with the global political economy. Sheikh Mohammed bin-Rashid al-Maktoum and his businessmen-advisors turned Dubai into a regional trade hub with 13 free trade zones; financial, shipping, and media centers; and a 5 million annual tourist destination with an open stock exchange and outward-oriented appeal to foreign companies (800 from the United States alone by 2006). Dubai’s leaders, however, had strong incentives to embark on this path, given their early reading of fewer oil endowments than those of their neighbors. This reading highlights the importance of agency, sometimes overlooked by resource abundance arguments. I return to the “oil curse” when discussing rational explanations later. Dubai’s alternative path reduced its oil dependence to 6 percent of state income, a feat that its neighbors began to emulate. Whether these shifts would have been possible in the absence of massive inflows of Arab capital remains subject to empirical investigation. For our purposes here, however, one must note the role of an external structural

shock – 9/11 and lukewarm responses to Arab investments in the West – in redirecting Arab capital to the most lucrative Middle East sites. A final challenge to deterministic oil-curse explanations is provided by Malaysia and Indonesia, forcing an analytical move to choice arguments or to alternative structural perspectives.

A final structural argument in this brief overview might explain the adoption of alternative models in each region by focusing on diffusion, a recently revived outside-in causal mechanism that Collier and Messick (1975) had called attention to but that, according to Jahn (2006), has been all too long ignored. Diffusion – concomitant outside-in effects on a cluster of interdependent states – can be crucial in most-similar-systems designs. Diffusion cannot be explained by characteristics of states acting independently but rather by their interactions – the so-called Galton’s problem identified earlier – that may be particularly severe in studies of contiguous states and regions. Thus, in the Middle East, diffusion could be found in the progressive adoption of Nasserite or Ba’athist models, which was often accompanied by forceful external coercion and intervention (agency) from within the region but also by internal collaboration.³⁶ Nasser threatened and subverted liberalizing efforts by small, resource-poor Jordan and Lebanon, among others (Iraq, Yemen), portraying them as antirevolutionary retrogrades and enemies of “Arabism,” decrying their association with Western powers and markets, recommending expulsion from the Arab League, and mobilizing pan-Arab nationalist domestic forces within target states to replace their leaders. Syria and Iraq’s Ba’ath often coerced Jordan to tow the line, and Lebanon’s export-oriented model, steered mainly by dominant Christian (Maronite) elites, also faced Nasserite and Syrian challenges. Syria’s occupation of Lebanon ended after nearly 30 years, but not Syria’s efforts to subvert sharp departures from its reigning model.

Like other structural arguments reviewed here, this one may also be complemented by strategic interaction considerations. Leaders in Jordan, Lebanon, and elsewhere may have had incentives to follow economic and political rationales akin to those of East Asian leaders but, confronted by coercion and diffusion effects from the regional environment – in the form of forceful, intrusive, inward-looking pan-Arab models – read their prospects of political survival *at home* as foreclosing exits from extant models (Lust-Okar 2005). In East Asia, progressive diffusion of successful export-oriented models – beginning with Japan – arguably predisposed successive East Asian regimes to adapt analogous models to local circumstances in Taiwan, South Korea, Hong Kong, Singapore, Malaysia, Thailand, China, Indonesia, and Vietnam. This diffusion was famously captured initially by the “flying geese” analogy led by Japanese capital and technology through foreign direct investment (FDI) and bank loans, followed by “tigers” and “dragons” (MacIntyre and Naughton 2005).

³⁶ Lustick (1997). Halliday (2005: 68) refers to broader processes of imitation and competition in the Arab world as “emulative linkages” or “emulative homogenisation.”

These and other theories cast at the international level seem closer to resolving Galton's problem but also leave problems of agency, causal mechanisms, and regional contextuality – as well as fallacies of composition, division, questionable or single cause, or hasty generalization – unanswered. Rationalistic and cultural understandings help solve at least some of these problems.

Rational Sources. Structural arguments' open-endedness leads to the search for complements and alternatives with more focused accounts of key actors' incentives, interests, preferences, choices, and unintended effects in a context of strategic interaction with other domestic and external actors (Levi this volume; Lake and Powell 1999). One such complement positions rational leaders at critical decision junctures, facing clear threats and dilemmas of survival that required the construction and maintenance of supportive domestic coalitions. As argued, the 1950s–1960s were rife with crises and junctures that demanded clear identification and assessment of appropriate political allies, their potential incentives and resources, and compensatory mechanisms for potential losers. This domestic context required proper strategic reading of the availability and strength of coalitional partners favoring one model or the other, based on those partners' incentives regarding forms and levels of integration in the global economy. East Asian leaders were arguably able to promote private capital oriented to the global economy because potential opponents of that model had been weakened, largely by exogenous outside-in effects. World War II had undercapitalized and disorganized industrial groups, and land reform following decolonization from Japan, the KMT takeover in Taiwan, and the Korean War had decimated agrarian elites.³⁷ Both the U.S. occupation and the KMT's reliance on technocrats enhanced leaders' autonomy from private domestic elites. The short duration of import-substitution in Taiwan prevented political encroachment by beneficiaries, facilitating support of export promotion by mid-sized and smaller firms.³⁸ Park's shift toward an export-led strategy in 1964 could hardly be opposed by ineffective import-substituting interests, co-optable into the new model through pressure and inducements (Haggard 1990).

By contrast, early import substitution in the Middle East had created very powerful constituencies with few incentives to support different models. Landed aristocracies from Iraq to Egypt remained in place after World War II. Nasser's 1950s land reform aimed at empowering poor and landless peasants but, unintendedly, benefited middle and rich peasants instead, achieving limited redistribution and failing to eliminate "the political phenomenon of landed wealth itself" (Binder 1978). Larger landowning families (albeit not the largest prerevolutionary landowners) controlled nearly half of the local councils and undermined the purposes of Nasser's Arab Socialist Union (Migdal 1988). State accommodation with rural strongmen enabled resource transfers from rural to urban needs, a pattern common throughout the region. Whereas East Asian leaders sought

³⁷ Evans (1995); Stubbs (1999); Haggard (1990); Chan and Clark (1992).

³⁸ Chan (1988); Cheng (1990); Cumings (1984); Moon (1994).

rural political support through effective land reform and agriculture and rural infrastructural investments, the Middle Eastern countryside – particularly in revolutionary states – was largely forced to support narrow urban coalitions and subsidize their consumption (Campos and Root 1996: 3–6). Rural interests were no political barrier to export-led models in East Asia, and land reform helped level income distribution (Haggard 1990; Chan 1990). Middle Eastern regimes diffused the threat of peasant insurgency through state monopolies, East Asian regimes through export-led growth. Above all, the Middle Eastern military was a formidable opponent of privatization as the “new class” and main beneficiary of nationalization (Halliday 2005).

As argued earlier, Doner et. al.’s (2005) outside-in argument is sensitive to both structure and rationality, tracing the rise of impressive developmental states in Northeast Asia to “systemic vulnerability” (extreme geopolitical insecurity and severe resource constraints). Though advancing *prima facie* an international structural explanation for the nature of domestic structures (states, institutional capacity), they also analyze how rational leaders, reading the contours of their situation and deriving logical inferences about how to survive politically, adopted export promotion and industrial deepening. Political elites built effective developmental states while “simultaneously staring down the barrels of three different guns”: (1) credible threats that deterioration in living standards of popular sectors could trigger unmanageable mass unrest; (2) heightened need for foreign exchange and war matériel induced by national insecurity; and (3) hard budget constraints imposed by scarcity of easy revenue sources. These interactive conditions are the essence of “systemic vulnerability,” which enabled bureaucrats in South Korea, Taiwan, and Singapore to solve information and commitment problems, coordinate multiple actors (state and private), and pursue long-term economic objectives.

Yet, this sophisticated interpretation of how East Asian leaders and ruling coalitions defined political survival in terms of a particular export-led growth model falters when applied to the Middle East, where at least some ruling coalitions stared down the barrels of guns similar to those of their East Asian counterparts but responded differently. Arab leaders faced dangers of internal unrest and dissatisfaction and external threats stemming from the Cold War, each other (Kerr 1971), and Israel. Some could cope with massive acquisitions of war matériel through oil resources but others could not, yet both faced guns-versus-butter dilemmas, particularly in the 1950s–1960s, prior to the sharp rises in oil revenues. With or without those resources, most built gargantuan military-industrial complexes, indulged in the highest levels of military expenditures relative to GDP and to government expenditures in the industrializing world, and sustained the largest militaries relative to population.³⁹ Military elites accustomed to rents were major opponents of privatization (Halliday 2005: 55; Henry and Springborg 2001). Thus, while staring down the barrels of similar

³⁹ Arms Control and Disarmament Agency; Bill and Springborg (2001: 171–172); Richards and Waterbury (1990: 354, 362); Solingen (2007b).

guns, Middle East leaders read incentives differently – partly on the basis of available political allies but plausibly also because of normative considerations discussed later – eschewing export-led models.

Our earlier overview of rationalist literature also discussed the relationship between leaders' political survival and war. A particular case of outside-in effects related to external threats is defeat in war, which can fuel crises and catalyze departures in models of political survival. Leaders from South Korea to Taiwan to major Arab states endured war experiences in the late 1940s/early 1950s, leading to various removals from office. Import-substitution ensued in both regions in the wars' immediate aftermath, yet East Asian leaders moved on to enforce export-led growth subsequently. Beyond other conditions explaining that shift reviewed earlier and later, leaders had also learned different lessons from defeat in war. The KMT associated its 1949 defeat by mainland Communists with its own record of hyperinflation, hyperinequality, and hypercorruption (Chan and Clark 1992). Hence, Taiwan's new model was pivoted on price stability, egalitarian income distribution, and decentralized (small-medium) private entrepreneurship. Park's lessons from North Korea's 1950 overrun led him to reject a coalition of absentee landlords and corrupt import-substituting industrialists backing his predecessor's (Rhee Syngman) model, under which a weakened South Korea was almost entirely swallowed by the North. The KMT's complete ejection from the mainland and South Korea's nearly complete invasion by the North were arguably more devastating *territorially* than the Arab states' 1948 defeat. This war, which Arab states initiated while rejecting the UN partition of Palestine, did not result in complete territorial ejection, as with the Chinese nationalists in 1949 and South Korea in 1950. However, the *Nakba* (cataclysm) dealt a severe blow to Palestinian and pan-Arab aspirations, coloring the strongly nationalist pan-Arab (*kawmīyah*) models that emerged in the 1950s. Despite Israel's depiction of the war as a "victory," the loss of 1 percent of its population in a war fought under an arms boycott also left a legacy of inward-looking self-reliance among first-generation Israeli leaders.

Over two decades later, Anwar Sadat portrayed the 1973 October/Yom Kippur War as an Egyptian victory, enabling him to launch an *infitah* (economic opening) in 1974. The victorious "crossing" of the Suez Canal would facilitate the "crossing" from Nasserism to a brand new model. Israeli leaders depicted the October war as a victory despite the shock of a misread surprise attack on its holiest day (Yom Kippur) and its high number of casualties. To be sure, there were new lessons learned regarding strategic and economic vulnerability. In sum, these examples suggest that all leaders defined victory and defeat in self-serving ways (Johnson and Tierney 2006). Defeat in war was common across both regions and was not necessarily a precursor of inward-looking models or perennial armed conflict among neighbors. Leaders manipulated external debacles in ways that reinforced their domestic agenda, steering them to support one model or another. Finally, threats of peasant-based Communist revolutions emanating from neighboring states (mainly China) are sometimes invoked to explain East Asian leaders'

economic choices (Amsden 1989). Rural reform and broad economic growth would arguably deflate such threats, helping leaders survive in power. Similar revolutionary pressures did not lead to analogous decisions in the Middle East.

In a stimulating analysis, Halliday (2005) provides a far more nuanced approach directly relevant to the adoption of Middle Eastern models, one sensitive to structural influences but pivoted on rulers' dilemmas of political survival, stemming from their strategic interaction with other actors, bureaucracies, societal and institutional civilian and military forces. This approach conforms with findings in the previous section that domestic political factors can sometimes be more resilient than hyperstructuralist paradigms suggest and that simplistic international or domestic structural reductionism is being superseded by more sophisticated analyses of the interaction between the two.

Culture. Theories of culture, insofar as they frame the context of politics (Ross this volume) endogenize models of political survival but have not been systematically deployed to explain the particular puzzle at hand. Paradoxically, despite enormous intraregional diversity, both regions shared certain cultural similarities emphasizing family, literacy, and community. An earlier literature found Confucianism to be the main barrier to outward-looking modernization in East Asia, later superseded by a literature that imputed precisely the opposite effects to Confucian beliefs. Not only was this cultural interpretation of economic choices in East Asia elusive but, when transposed to the Middle East, posed further difficulties. If cultural constructs of family, literacy, and community could explain the choice for rapid socioeconomic change in East Asia, why would similar cultural constructs sometimes associated with Middle Eastern societies not have comparable effects? In studying culture, Galton's problem – interdependence of cases in cross-cultural analysis – at the regional level could arguably be more severe in the Middle East due to higher geographical contiguity and common language, in contrast to East Asia's geographical spread across waterways and vastly distinctive languages.

Many consider the analytical move away from essentialist perspectives to constructivism as a progressive development with potential for providing improved explanations for the adoption of different models of political survival. A large body of constructivist work emerged in the past decade, providing better understanding of nuances and contextuality of Islam, Confucianism, and other cultural forms. Yet, Halliday (2005: 12) detects a progressive attenuation of substantive research on Middle Eastern societies and unresolved circular debates on methodological issues akin – in Halliday's eyes – to what Geertz termed "epistemological hypochondria." Halliday also laments what he considers the most dominant, entrenched, misconceived, and diversionary regional epistemological combat of all – that between "Orientalists" and their critics – treating this debate as one dominated *on both sides* by an introspective U.S. academic narcissism and recommending, instead, the superior work of Maxime Rodinson, an unabashed Marxist Orientalist. Halliday suggests that neither the debate

on “culture” and “difference” nor the one on Orientalism “yielded much epistemological progress of any general help to social science” (2005: 12). Whether or not that is indeed the case, a sweeping overview of this debate in the social sciences is not this chapter’s subject.⁴⁰ The exercise here is restricted to extracting outside-in and inside-out effects from the literature – in this case on culture – that might help explain the choice of different models in both East Asia and the Middle East. The regional “exceptionalism” bemoaned by Halliday provides a particular barrier to understanding here, unsurprisingly since cultural studies often reinforce the “particular.”

Nonetheless, progress made along constructivist lines in the more general comparative and international politics literature provides potential lines of argumentation. Abdelal’s (2003) work on inside-out effects, discussed earlier, argues that nationalism influences economic approaches to regional integration. Accordingly, national identities vary in (1) content (who is a member, purposes of statehood and economic activity, what “other” states threaten such purposes) and (2) contestation (how much disagreement exists regarding nationalist definitions of economic purpose). Shared national identity, in Abdelal’s view, engenders the economic sacrifice necessary to achieve societal goals and to realize the nationalists’ longer term vision, lengthening time horizons. Yet, he also acknowledges that national identity is sometimes contested or ambiguous. Indeed, one might argue that it is a rare case when it is not contested. Virtually every Middle Eastern state is a case in fact. Nationalism at the individual Arab state level (*watanyiah*, or *raison d’état*) was a highly contested construct, entailing vastly disputed goals and visions (Halliday 2005: 62). For some it meant advancing the narrow interests of ruling coalitions; for others the interests of ethnic or tribal groups; and for yet others, *watanyiah* was only a barrier to a higher brand of nationalism, the pan-Arab variety (*al-qawmiyyat al-Arabiyya*, or *raison de la nation*).

For all these variants, visions ranged from highly autarchic concepts of nationalism to openness to the rest of the world. Shared national identities regarding societal goals were hard to distill not only for more extreme cases such as Lebanon in 1953, 1978, or even 2007 but also for an overwhelming majority of Middle Eastern states – Arab, Iranian, Turkish, or Israeli. Nor were spreads over the content of nationalism narrower among East Asian states, from Japan to Taiwan and Indonesia. Conceptions of goals and visions have been wide-ranging even in homogeneous “national states” such as Korea and Japan, leaving ample margins for political leaders to forge a national consensus of their choice, by force or persuasion. Yoshida Shigeru fashioned a compromise that became the focal point of postwar national Japan, including economic goals (Samuels 2003). Park Chung Hee fashioned another compromise, specifically receptive to a Japanese economic role in the construction of a new national myth – Korea’s economic miracle – a role that could hardly be conceived as flowing naturally

⁴⁰ Anderson (2003), a leading Middle East expert, commends the insights from literary criticism while lamenting an overemphasis on culture and an absence of significant empirical social science on the Middle East.

from Korean national historical memories. The progressive broadening of beneficiaries from such compromises in various East Asian states, and compensatory mechanisms for losers, had much to do with the durability (equilibrium) of those compromises. And yet, few would characterize South Korea's "national economic purpose" as coherent even today. The U.S. alliance and a landmark 2007 free trade agreement (FTA) with the United States are hardly an expression of a strong consensus or coherent national purpose.

Abdelal (2003: 930) argues that "when societies agreed on the social purposes of their new states, purposes that motivated economic sacrifice, the outcome was economic nationalism – a rejection of regional reintegration." Models adopted by Japan, South Korea, Taiwan, Vietnam, and much of East Asia do not easily conform to this expectation, as each became economically oriented to – not tilting away from – presumed "rival" identities such as those of the United States, Japan, and China, respectively. Indeed, even China – best positioned to lean on a huge domestic market – came to cast its economic fate with U.S., Japanese, and Western markets, capital, and technology. Historical memories fueling Chinese nationalism could be more easily reconciled with Mao's autarchic principles than with Deng's turn to an export-oriented model that made China far more dependent on purported "foes," at least in the short term (Zweig 2002). How "national purposes" were constructed in each case, across both regions, is a subject beyond this chapter's scope, but it's worth noting that myths of "self-reliance" were historically shared by actors advancing export-led growth no less than by actors advancing autarchy. A partitioned Korea, with each half embracing radically different models, illuminates how respective national myths emerged through coercion and indoctrination. Kim Il-Sung brutally imposed national purpose on North Korea, physically eviscerating competing renderings of revolutionary identity, with different implications for economic policy (Cumings 1990). The result was "self-reliance in the economy [*juche*]" as "the material basis of *chajusong*" (all-round independence in international relations), for "economic dependence leads to political subordination."⁴¹ A "self-dependent national economy" (*charip kyongje*) would produce virtually everything at home, with the Soviet Union filling some minor gaps. Through *juche*, Kim Il-Sung, the Korean Workers' Party (KWP), and the North Korean People's Army would deliver invulnerability to the Korean people, a highly valued sentiment for a population subjugated to ruthless Japanese rule for decades. *Juche* was the path to *minjok tongnip* (national or ethnic independence) and to disparaging Kim's domestic opponents, including pro-Soviet and pro-Chinese factions, embodying a *nationalist* principle that replaced Marxism-Leninism and served as the basis of a personality cult and a moral compass from which to belittle South Korea's model as *sadaejui* (mercenary, flunkey, "puppet" of imperialism).

Despite South Korea's alliance with the United States, its national purpose was certainly not cohesive across the regimes of Rhee Syngman and Park Chung Hee. The former included extreme nationalists – Yi Pom Sok, the first minister

⁴¹ Smith (1994: 100); Mikheev (1996: 92); An (1983: 35–71).

of defense and prime minister – who exalted the nation and the state (*minjok chisang*, *kukka chisang*, or nation first, state first), as well as racial purity (*hyolt'ong*), while opposing reliance on foreigners (*sadaejui*), in ways that summon not only 1930s-style European fascism but also Kim Il-Sungism (Solingen 1998). The 1948 constitution was inspired by Friedrich List but also by Karl Marx (Amsden 1989). Rhee rejected U.S. pressures for privatization and conservative fiscal and monetary policies likely to undercut his patronage system. Park, by contrast, imposed the mottoes “Nation Building Through Exports” and “Think Export First!” (Ogle 1990: 40), pointing that “for a country like Korea, unendowed by nature and saddled with minuscule markets, only an external-oriented development strategy, making full use of the abundant human resources but aimed at exports, appeared relevant” (Park 1979: 72). Park sought to forge national purpose around a purported economic miracle, through coercion and persuasion, leading to growth in manufactured goods from 17 percent of all exports (early 1960s) to 83 percent a decade later.⁴² Trade openness (exports plus imports) doubled from 20 percent of GDP in 1963 to 40 in 1970, in a mere seven years. Park (1971, 1976, 1979) hailed the “crossing” of the \$10 billion threshold in manufacturing exports (1977) as a milestone in a strategy that had lifted South Korea’s economy from 72nd (1962) to 28th place worldwide (1976), carefully emphasizing that South Korea had achieved that target more quickly than West Germany and Japan. Yet, during the 1970s economic crisis, Park introduced reforms wrapped in *juche* and national identity rhetoric. Leaders on both sides of the 38th parallel could thus uncannily manipulate Korean national identity to advance competing models on the basis of a common pledge to *juche*.

Another concept emphasized in the past decade by work on culture is that of sovereignty, lying at the heart of domestic–international linkages. Krasner (1999) defined Westphalian sovereignty as the exclusion of external actors from internal authority structures. Social mechanisms – instrumental and otherwise – may be at work (Zuckerman this volume) in explaining which external actors are included in/excluded from internal authority structures and why. Could different constructions of sovereignty norms account for variable receptivity to different models of political survival in each region? Sovereignty norms are considered to have taken hold in East Asia only during the late twentieth century (Chan 1999: 200), although Hui (2005) finds them to be rooted in much earlier periods for China. On the one hand, sovereignty gained greater ascendancy in recent decades as a norm governing *intraregional* relations, possibly contributing to the decline of war and military interventions throughout the region. A more stable and cooperative regional order was certainly a precondition for undertaking mutually reinforcing export-oriented models capable of attracting foreign investment, capital, and technology. On the other hand, these models also required significant compromises of Westphalian sovereignty to accommodate markets and institutions in a global political economy. Indeed, models of engagement with the global economy played significant roles in the willingness of East Asian leaders in Japan, Taiwan,

⁴² Park interview with *Time* (June 30, 1975).

South Korea, and Southeast Asia to even compromise sovereign rights to acquire nuclear weapons, even when other powers in the region had already gone nuclear (China, the Soviet Union/Russia, and North Korea) (Solingen 2007a). Insofar as compromised Westphalian sovereignty in East Asia facilitated global economic access, one cannot exclude the possibility that such compromises were the result of prior domestic decisions to adopt these models rather than their cause. Additional research can shed light on the mutual interactions between domestic and international, as well as between cultural and rational, explanations for the nature of compromises over sovereignty.

Sovereignty norms exhibit a contrasting trajectory in the Middle East, in a clear instantiation of Krasner's understanding of sovereignty as "organized hypocrisy," another social mechanism. On the one hand, conceptions of national economic purpose throughout the region were highly contested from within and without new sovereign states. Pan-Arab (*qawmiyyah*), and later pan-Islamic, identities weakened the legitimacy of sovereign statehood (*watanyiyah*). Both were, in different contexts and at different times, convenient tools for camouflaging minority control of military and state institutions, as with Syria's Alawi, Saudi Arabia's Sudairi, Jordan's Hashemite, and Iraq's Tikriti minorities.⁴³ The state was not the highest locus of political identification and legitimacy in the Arab Middle East (Ibrahim 1995), where leaders blamed colonialism not for incorrect border demarcation but for conceiving of borders at all (Gause 1992). This enabled mutual challenges of sovereignty norms of noninterference, including subversion of neighboring rulers with competing interpretations of those norms, cross-border militarized conflicts, calls for political unification schemes, and violent efforts at ideological homogenization of other states. This (outside-in) contested approach to state sovereignty enabled intrusion into neighboring states, casting a shadow on leaders' independent ability to adopt favored models of political survival. In contrast to the downgrading of sovereignty norms in *interregional* relations (Kerr 1971; Halliday 2005), various Middle Eastern rulers upheld far more rigid concepts of sovereignty when it came to defining relations with *global* economic and political institutions. Here there were fewer Westphalian compromises than in East Asia and more intense rhetorics of self-reliance vis-à-vis the rest of the world. Subsequently, pan-Islamism replaced pan-Arabism as the key foe of sovereign statehood, a transnational challenge that began afflicting Southeast Asia but to a much more limited extent.

Summing up, East Asian states arguably upheld state sovereignty norms at the regional level but relaxed them with respect to the global political economy. Most Middle Eastern states tolerated diluted sovereignty norms in inter-Arab relations but imposed far more stringent versions when defining relations with the global political economy. These contrasts may help explain broader

⁴³ For Halliday (2005: 35) norms are epiphenomenal, invoked primarily for "political calculation." Yet, such calculations – regarding *qawmiyyah* and *watanyiyah*, for instance – can also be seen as contextually derived and intersubjectively held.

receptivity to different models of political survival – internationalizing, inward-looking – in each case. This provides one plausible account of the independent impact of contextual, constructed norms on the processes of interest and preference identification and aggregation. At a more general level, this overview of cultural dimensions concurs with Ross’s (this volume) suggestion that “the effects of culture on collective action and political life are generally indirect, and to fully appreciate the role of culture in political life, it is necessary to inquire how culture interacts with and shapes interests and institutions.”

The Consequences of Models of Political Survival

Structural Consequences. Models of political survival – and their associated outside-in effects – had important intended and unintended consequences for domestic institutions, social actors, and approaches to regional relations. This section focuses on a selected few, arranged according to flexible paradigmatic formulations. To begin with, how did competing models affect state structures? Although states played central roles in both models, the extent to which states replaced or enhanced private capital differed significantly. East Asian leaders mandated state institutions to provide lending, subsidies, and other incentives to private firms that met export performance goals. Their dependence on export-led growth steered state intervention toward ensuring macroeconomic stability, new markets, investment in growth areas, and stable political-economic environments for foreign investment (Noland and Pack 2005). There were wide differences between Japan’s “minimalist state” (in overall size) and virtually every other East Asian state form, but extensive consultations with private firms were common even if states enjoyed significant autonomy in the initial phases.⁴⁴ Even interventionist models, such as the KMT’s, exposed “greenhouse capitalists” to the rigors of the market, phasing out protection, enforcing export quotas, and limiting state entrepreneurship (Evans 1995: 57). East Asian firms – not states – made most decisions about resource allocation, while states supported export-oriented industries by providing credit and promoting technical skills without micromanaging or thwarting private enterprise (Stiglitz 1996).

Thus, East Asian states were active lenders and regulators but dramatically less active entrepreneurs than Middle Eastern states, which presided over massive nationalizations of oil, banking, industry, and the Suez Canal, channeling proceeds as monopoly rents. These rents enabled leaders to introduce expansive yet ultimately unsustainable populist programs while promoting import-substitution. As in other cases, redistribution dissipated within a decade after Egypt’s revolution (Waterbury 1983: 8). Import-substitution, with its compelling logic and inherent weaknesses (Hirschman 1986; Bates 2001), also involved income transfers from agriculture to infant industries under state sponsorship. Together with forceful suppression of private firms, leaders eliminated economic and political

⁴⁴ Haggard (1990, 2004); MacIntyre (1994); Okimoto (1989); Pempel (1998).

competitors to the state (Anderson 1987; Binder 1988). In rentier states, “there was, in effect, no such thing as a private sector” (Halliday 2005: 278) and the oil bonanza (of the 1970s) strengthened state sectors further. State expansion – not exposure to the global economy – depleted private capital of economic and political strength, albeit to a lesser extent in Jordan, Morocco, Lebanon, and Turkey than in Egypt, Syria, Algeria, Yemen, Libya, and Iraq. Many states adopted import-substitution in the 1950s–1960s, but few embraced as comprehensive statist controls, state entrepreneurship, and large bureaucracies as most states in the Middle East did.⁴⁵

States played a critical role in defining relations with the global economy in both cases but were also transformed by the consequences, intended and unintended. East Asian leaders vigilantly steered states into macroeconomic stability and proper conditions for sustained export-led growth, turning states into supply mechanisms connecting domestic with global markets, or approximations of *developmental, trading, and virtual* states.⁴⁶ Middle East leaders crafted states bound to deplete and ossify, presiding over current account and budget deficits, inflation, and scarce foreign exchange, and ultimately too weak to exert control over society except through force, or typical *predatory* states.⁴⁷ Developmental states were handmaidens of industrial transformation, whereas predatory states undercut development even in the narrow sense of capital accumulation.⁴⁸ The former relied on Weberian-style meritocratic bureaucracies effectively extracting resources from society, whereas such bureaucracies were largely absent in rentier economies (Weinthal and Luong 2006). Neither state form was free from cronyism and corruption, but only the former delivered “collective goods.” Military expenditure ratios explain some of these differences. Nor was either state model invulnerable, although they faced different challenges. East Asia’s models were more susceptible to the global supply of, and demand for, manufactures and the evolving risks of capital liberalization, yet the 1997 financial crisis ultimately proved their resilience. Middle Eastern states were vulnerable to the exhaustion of import-substitution, balance-of-payments problems, inflation, unemployment, inefficient industries, growing inequality, weak private enterprise, and oil cycles.

Models of political survival, and their implications for state forms, also affected the role of the military. Each model bore different intended and unintended consequences for the relative size of military-industrial complexes, the extent to which these replaced private enterprise, and their short- and longer-term viability. Many of the failed public enterprises and beneficiaries of closure propped up by sclerotic states throughout the Middle East were part of expansive military-industrial complexes entailing far more than armament production. These sprawling networks replaced private firms in most economic sectors, under high

⁴⁵ Richards and Waterbury (1990); Owen (2000); Henry and Springborg (2001).

⁴⁶ Woo-Cumings (1999); Rosecrance (1999).

⁴⁷ Anderson (1986); Migdal (1988); Waterbury (1993).

⁴⁸ Wade (1990); Evans (1995); Amsden (2001).

tariffs and protection, absorbing even more resources than the already high direct allocations to the military.⁴⁹ Average military expenditures/GNP reached 15–25 percent (1970s–1980s), several times the industrializing world’s mean (5 percent), with notable exceptions among the few Middle Eastern states pursuing export-led models, such as Morocco and Tunisia. Middle Eastern military expenditures relative to GNP were three to four times East Asia’s average in recent decades (Solingen 2001), absorbing significantly higher shares of budgets. The Middle East still accounts for six of the highest eight military expenditures/GNP spenders worldwide (Halliday 2005: 337). By contrast, East Asian growth models required stable macroeconomic policies, predictable environments, and minimizing inflationary military allocations that might endanger the models’ core objectives. As export-led models consolidated, military expenditures as a proportion of GNP and central budgets declined. Only one decade after barely surviving North Korea’s attack, South Korea *reduced* military expenditures/GNP from 6 percent under Rhee (1960–1961) to 4 percent (1963–1975) under Park’s model, even as U.S. military aid was phased out (the presence of U.S. troops was surely related to that decline, but the size of U.S. troops also declined over time). Even Taiwan’s ratios declined despite unabated external threats, in line with KMT leaders’ reluctance to finance expensive indigenous weapons industries that might imperil export-led growth (Chan and Clark 1992; Cheng 1993). Growing economies enabled higher absolute military expenditures, but their lag after GNP growth and lower budget percentages than average for industrializing states suggested some moderation. The result, as intended, was “rich states, strong armies” in East Asia and the unintended weakening of military capabilities for external defense in the Middle East.

A final structural argument links models of political survival, and their implications for state forms and military institutions reviewed in preceding paragraphs, to the democratic or authoritarian nature of regimes. U.S. intervention is often invoked as a source of authoritarianism, inspired by landmark events such as the U.S.–British coup against Mossadegh in Iran in 1953. Several such interventions around the world during the Cold War support the argument’s plausibility. At the same time, reducing the incidence of authoritarianism to U.S. intervention is difficult to sustain in light of the disparate outcomes in both regions: a democratizing East Asia and an intractably authoritarian Middle East. U.S. meddling – indeed, physical occupation in many cases – in East Asia did not prevent widespread democratization from Japan to Taiwan, South Korea, Indonesia, the Philippines, Thailand, and others. One might also examine broad structural/class alliances leading to different institutional outcomes in each region, in the tradition of Barrington Moore (1966) and Luebbert (1991). Our focus here, however – more specific to the implications of models of political survival for the development of democratic institutions – can benefit from a closer look at the rational incentives of different domestic actors *as built into the models*, and their intended and unintended effects.

⁴⁹ Waterbury (1993); Owen and Pamuk (1999).

Rationality. The nature of institutions connecting states and markets (domestic and foreign) cast their shadow on the prospects for democratic development. One standard theory holds that the freer operation of markets advances the development of democratic institutions. The East Asian cases inspired other theories, such as “authoritarian advantage,” as better suited to advance economic reform through repression. Yet, this theory looked only at successes rather than average performance (Przeworski et al. 2000) and could hardly explain why authoritarian Middle Eastern leaders faced different incentives regarding democratization or failed to act on presumed incentives for economic reform. At least part of the explanation for these differences relates to distinct incentives in each case, stemming from the strategic interaction between rulers and military institutions. The latter played important roles as repressive mechanisms of control in both regions and benefited from overall high military expenditures during the Cold War. Yet, although both models relied on military-authoritarian institutions, the implications of each model for the role of the military and private entrepreneurship differed, and with them the implications for authoritarianism.

East Asian leaders did not design export-led models to advance but to curb democracy. However, the models’ unintended effects arguably encouraged democracy via three mechanisms. First, they fostered stronger private sectors and civil societies that eventually overturned authoritarianism in most cases. Second, the requirements for macroeconomic stability and reduced state entrepreneurship limited military complexes and undermined the military’s ability to develop independent resources, forcing it to evolve into more professional forces less hostile to groups underwriting outward-oriented growth. Third, the models led to unprecedented economic growth. Since democracy is assumed to be least reversible where annual per-capita income rises above \$6,000 (Przeworski et al. 2000), growth can partially explain democracy’s resilience in these cases. At lower income levels, economic growth with low/moderate inflation – an important ingredient in East Asian models – heightens democracy’s probability of survival. These three mechanisms operated in Taiwan, South Korea, the Philippines, Thailand, and Indonesia, which evolved into democracies, whereas Mahathir Mohamad truncated them in Malaysia under an “Asian way” banner justifying authoritarianism. China and Singapore remained chief outliers (export-led models in Indochina are more recent, and their effects may not be evident yet). Despite these anomalies, East Asia’s modal trajectory has arguably helped transform export-led growth into democratic polities.

By contrast, inward-looking Middle Eastern models arguably created three built-in barriers to democratic development.⁵⁰ First, by undermining independent private sectors, they deprived democratic forces of key potential allies; weakened civil societies could not rely on capitalists’ demands for political reform. Furthermore, Binder’s (1988: 343) statement that “the rhetoric of

⁵⁰ For an overview of barriers to democratization in the Middle East, see Posusney and Angrist (2005).

political liberalism in Egypt does not include capitalism” applied more broadly. Consequently, efforts by Middle Eastern leaders to adopt export-led models, such as Sadat’s *infitah*, had to contend with well-entrenched protectionist private interests resistant to overturning partnerships with military and state enterprises, which were also particularly strong in Syria and Iraq. Second, import-substitution involved a far more fatal embrace with the military, endowing the latter with vast economic fiefdoms and resources. Repressive militaries thus had strong incentives to protect the authoritarian status quo (Bellin 2004). Military institutions remain the pivot of equilibria in Middle Eastern models. Unsurprisingly, modest steps toward democratization materialized only where private entrepreneurs were stronger and military-industrial complexes were weakest (Jordan, Morocco, Lebanon, and recently some Gulf states), providing some support for “democratic efficiency” theories that leaders are better off democratizing – even if at slow rates – as they reform economic models (Remmer 1989; Maravall 1994). Third, rentier states were sustained by different outside-in effects, relying on oil revenues to coopt populations without taxing them and turning many into rent seekers dependent on state subsidies in exchange for political acquiescence. Thus, rentierism too led to omnipotent states presiding over uncompetitive industries, suppressed labor, and undemocratic structures. Beblawi and Luciani (1987: 16) found that rentierism afflicted most Arab states, including oil-scarce ones that, as recipients of aid or remittances from oil states, endured “second-order” effects of Dutch disease and, with them, higher barriers to development of democratic institutions (Solingen 2007b).

The intended and unintended consequences of models of political survival for states, the military, and authoritarian institutions had, in turn, important implications for regional conflict. In the Middle East they led to a series of wars that can be explained by the following causal (outside-in and inside-out) mechanisms connecting leaders’ and ruling coalitions’ models of political survival to external conflict.

First, Middle Eastern leaders launched import-substitution to achieve rapid industrialization, robust entrepreneurial states, decreased reliance on international markets, and redistribution. Yet, import-substitution unintentionally depleted states’ resources and ossified the political machinery – often the military – that controlled them. Leaders of drained and entropic states were unable to deliver resources and services to constituencies previously mobilized through revolutionary or nationalist fervor. Hence, external conflict and nationalism became effective substitutes for maintaining political support and deflecting opposition. Lacking institutional power and legitimacy domestically and regionally, Middle Eastern leaders arguably deployed violence at home and abroad (Dodge 2002: 177). This domestic fragility – hidden behind pan-Arab rhetoric – fueled mutual assaults on sovereignty. As Halliday (2005: 291) contends, “in terms of the historical sociology of Charles Tilly . . . Middle Eastern states are in essence . . . based on the use and threat of force.” This brand of authoritarianism reinforced reliance on externalization of conflict as a tool to stifle domestic dissent, a standard inside-out effect or causal mechanism examined in this chapter’s first section.

Second, military-industrial complexes strongly wedded to import-substitution and state entrepreneurship replaced private capital as economic and political competitors. Inward-looking, weakened private sectors protected their partnerships with those complexes, which guaranteed their own survival. This strong convergence among state, private, and military institutional interests around import-substitution perpetuated sprawling, inefficient industrial complexes, often justified under the aura of nationalism, pan-Arab symbols, military prowess, and nationalist myths as instruments of political survival.⁵¹ As Glaser (2000) argues, when domestic interests rather than external threats lead states to build up military resources, ensuing arms races are more likely to lead to war. The same mechanisms – delegitimized *mukhabarat* authoritarian states with mammoth military-industrial complexes – afflicted both inter-Arab and Arab–Israeli relations (Kerr 1971). Yet, Israel provided an even more legitimate target from a pan-Arab perspective, and its own military-industrial complex, prowess, and excesses enabled continued investments in neighboring military complexes.⁵²

Third, pan-Arabism itself led to competitive outbidding among Arab leaders who threatened and attacked neighboring regimes in the name of “truer” versions of pan-Arabism (Barnett and Solingen 2007). External conflict – in classical second image reversed fashion – enhanced the military’s *raison d’être* and its prerogatives to gargantuan resources even under conditions of socioeconomic crisis. This was not only true for Saddam Hussein, who developed the Arab world’s largest military-industrial machine, putting it to work against Iran and Kuwait with the expectation that these wars would enhance both the military’s economic resources and Saddam’s own domestic survival via expanded control over Arabian Peninsula resources. It was also true for Nasser, whose military benefited from external confrontations in the 1950s (Halliday 2005: 56) and who diverted attention from the severe domestic economic crisis by launching a war on Yemen (1963–1965) with oil-rich monarchies as his chief targets; for Sadat’s gargantuan military expenditures in the early 1970s on war preparations against Israel, culminating in the 1973 war; and for Hafiz el-Asad, who externalized domestic unrest by threatening Jordan militarily (1970, 1980), attacking Israel (1973), and occupying Lebanon. This 30-year occupation benefited Syria’s

⁵¹ Bill and Springborg (2000); Dawisha (2003).

⁵² This is different from arguing that Arab military-industrial complexes were mere by-products of this factor. As the *Arab Human Development Report* (2002: 2) states, Israel “provides both a cause and an excuse for distorting the development agenda,” serving “to solidify the public against an outside aggressor.” The many conflicts among Arab states in the 1940s–1950s were superseded by more interstate wars, militarized interventions and mobilizations, invasions, border clashes, and covert (violent) cross-border subversion involving Egypt–Yemen/Saudi Arabia (1962–1967), Syria–Jordan (1970), North–South Yemen (1972), Libya/Algeria–Morocco (1970s), Libya–Chad (1973–1987), Morocco–Polisario (1975–2000), Syria–Lebanon (1980s–2005), Iran–Iraq (1980–1988), and Iraq–Kuwait/Saudi Arabia (1990–1991), among others, and attempted coups against neighboring rulers. The Iran–Iraq, Iraq–Kuwait/Saudi Arabia, and Morocco–Polisario wars accounted for the bulk of total Middle Eastern war casualties since 1973 (Solingen 2007b).

military in various ways, providing it with Lebanon's free market to promote its economic monopolies in Syria (Halliday 2005: 56).

Not all wars, however, were by design. On the eve of the 1967 Six-Day War, Nasser arrogantly proclaimed a total war with a basic aim: "the destruction of Israel" (Migdal 2000: 174), and ordered the closure of the Straits of Tiran to Israeli passage and the removal of UN peacekeepers from Sinai. As Quandt (1996: 13) argues, it was Nasser who "took the initiative to turn a relatively quiescent front of the Arab-Israeli conflict into a battlefield," in an illustration of the rhetorical overreach of revolutionary leaders (Halliday 2005). Nasser thus unleashed processes that he could no longer control, involving Egypt in a war that he belatedly tried to circumvent (Dawisha 2003: 251). Israel's Premier Levi Eshkol was extremely wary of going to war – as demonstrated in a radio address that captured his breaking voice – but was responsive to some cabinet coalition members who declared Israel's choice to be "to live or perish" (Solingen 1998). These events are emblematic of how intentional and rational actions can generate collective outcomes and aggregate behavior that seem nonrational and socially suboptimal (Levi this volume).⁵³ Or, in Mansfield and Snyder's words (2005: 11): "Nationalists' mobilizing rhetoric may make war more likely by distorting the nation's view of the chances of success in a fight, or of the feasibility of reaching a compromise with the enemy." Authoritarian leaders may have particular incentives to "gamble for resurrection" (Goemans 2000), but democracies are not immune either.

Similar logics of intended and unintended effects were responsible for wars launched by North Korea (1950s) and other wars in Indochina in the 1960s–1970s, including Sukarno's military *konfrontasi* against Malaysia and the Sino-Vietnamese war. Yet, with the inception of export-led models, there have been no major wars in East Asia for several decades. Indochina has been largely at peace for nearly three decades, maritime Southeast Asia for four, and North-east Asia for five. Enduring resentment over aggression and colonial domination (mainly by Japan), as well as persistent historical, ethnic, religious, and territorial disputes, have been restrained as never before in recent history, and East Asian states have normalized diplomatic relations notwithstanding continued tensions over Taiwan and the Korean peninsula. Since 1965, the incidence of interstate wars and militarized conflicts was nearly five times higher in the Middle East than in East Asia (Solingen 2007b). There were five major Middle Eastern wars with at least 10,000 casualties since 1965 but only two in

⁵³ Suboptimality (devastating defeat) for Nasser was clear as the Six Day War became known in the Arab world as the *Naksa* (setback), subsequently depriving Nasser of revenue from the Suez Canal and Sinai oil fields (Lust-Okar 2005). On Nasser's acknowledgment that the Yemen War was a miscalculation on his part, and on the Six Day War as Arab nationalism's Waterloo, see Dawisha (2003: 235–236, 250). The Six Day War might have been considered an imposed war for Israel and a remarkable victory, but it saddled its leaders with huge unintended consequences for sustaining Israeli democracy, domestic control of extremist groups (Arab and Israeli), and external relations.

East Asia. Since 1973 ballistic missiles have been used in battle 10 times, with Middle Eastern states accounting for 8 instances, East Asia for none (Karp 1995: 45). Egypt, Iraq, Libya, and allegedly Iran used chemical weapons in war; no states have done so in East Asia since World War II.

These differences are at least partially traceable to models of political survival, through outside-in and inside-out causal mechanisms explored in this chapter, which can be summarized as follows: In an effort to divert attention from failed domestic models, economically depleted, entropic, crisis-prone, and delegitimized Middle Eastern regimes with large military-industrial complexes were prone to (1) emphasize nationalism and military prowess; (2) externalize conflict and “gamble for resurrection;” (3) exacerbate arms races; (4) wield transnational allegiances and regional assertion; and (5) undermine the regional state system. Each of these vectors individually enhanced the prospects for war and militarized incidents. Collectively – in second-image reversed fashion – they made them even more likely. War itself might not have been these regimes’ preference, but mobilizations, overt subversions, and cross-border invasions were certainly intended, though not always controllable. Logics (1) through (5) created a structural tendency toward militarized conflict that willy-nilly slid into militarized incidents even when these may not have been the leaders’ preferred outcomes. By contrast, East Asia’s developmental states required (1) contained military-industrial complexes and limited military competition; (2) regional stability; (3) domestic stability, predictability, and foreign investments; and (4) controlling arms races that might affect (1) through (3). Each of these requirements individually diminished the prospects for war and militarized conflict. Collectively – from the outside-in – they reinforced each other, making such prospects even less likely.

Culture. Cultural implications of models of political survival have attracted attention during the past decade, exploring – though largely indirectly – how inward-looking or export-led growth models influenced cultural trends, domestic institutions, and external behavior. Clearly, different levels and kinds of exposure to international influences help promote or stem the diffusion of external knowledge, culture, and norms into the domestic domain, through “second-image reversed” effects. Similarly, domestic cultures influence external relations contingent on the nature of “second image” links. Different models are thus bound to result in different cultural forms and conduits between the domestic and international realms.

The nature of regional relations can be considered both a source and a consequence of such effects. Arab states shared a common language but were also separated by diverse ethnic, tribal, linguistic, and communal differences, within and across states. Norms related to Arab identity did not preclude extensive inter-Arab conflict. Ironically, a far more diverse East Asia – in linguistic, ethnic, religious, developmental, regime type, and other terms – generated more regional cooperation. However, it is plausible that levels of regional conflict and cooperation were less related to relative cultural

homogeneity/heterogeneity than to models of political survival. Thus, inward-looking models may have imposed structural and strategically interactive constraints on what might have otherwise been potential proclivities to cooperate with cultural kin. However, some studies have also suggested that shared identity might have led to more rather than less conflict, as pan-Arab nationalism threatened leaders, ruling coalitions, and state sovereignty. Inward-looking Arab leaders wielded pan-Arab aspirations to enhance their popularity at home. Acting in the name of *wataniya* was bad form and bad politics (Barnett 1998). Yet, leaders also fought over the meaning of pan-Arab norms for Arab unity and relations with the world, and worked to frustrate pan-Arab schemes that reduced their autonomy and their rein on power. Furthermore, since states exhausted by import-substitution and militarization were not the highest locus of legitimacy and identification, violations of sovereignty norms were more frequent, from militarized border conflicts to calls for political unification and violent efforts at ideological homogenization. Regional cooperation “foundered on the fears of each state for its national sovereignty,” a legacy that offers “a partial explanation as to why war continues to be so prevalent in the region” (Dodge and Higgott 2002: 24–25). The Arab–Israeli conflict too “is rooted in the lack of acceptance both of sovereign non-interference, and of borders themselves across the region. Territorial disputes between Israel and its neighbors, and also between states in the Arabian Peninsula and on either side of the Gulf, all point to the continuous prominence of territory and military force in the region.”⁵⁴ Competitive outbidding among inward-looking models reinforced identity-based contestation over the meaning of pan-Arab norms (Barnett and Solingen 2007). Struggles for pan-Arab leadership unintentionally increased opportunities for conflict (Kerr 1971).

Stronger norms of sovereignty in East Asia may have enabled their typical models of political survival, but consensus over sovereignty should not be overstated, as challenges remained. These might be classified according to the particular sovereignty *problematiques* they raised: mergers, acquisitions, fair division, internal challenges, transnational identity networks, and sovereignty overhaul. First, China’s claims for merger with Taiwan remain strong, asserting that complete mainland sovereignty over both territories will be defended through force. Some groups in Taiwan demand complete sovereignty from the mainland, arguing that – as a formidably viable democratic entity in economic, political, and military terms – Taiwan’s lack of sovereignty illustrates fictions engendered by the concept of sovereignty. Second, North Korea’s potential acquisition by South Korea assumes collapse of the former, leading to a unified, sovereign Korea. Third, dilemmas of fair division of resources (particularly energy) – and hence contested sovereignty – remain over the Spratly Islands, Senkaku/Diaoyutai, and others. Fourth, domestic challenges to state sovereignty over its entire territory in Indonesia, China, the Philippines, Thailand, and elsewhere linger.

⁵⁴ As Halliday (2005) argues, Israel too (like Turkey and Iran) was drawn into a regional system with tenuous deference to norms of sovereignty.

Fifth, transnational identity networks – Islam in Southeast Asia or extensive Chinese ties – defy state sovereignty. Lastly, Japan is overhauling its self-induced limits on sovereign rights to develop military capabilities and become a “normal state.”

East Asian models of openness to the regional and global economies have helped transform cultural forms within states in the region via second-image effects, largely through diffusion of art forms from *manga* to song and film. East Asian societies have also contributed to global cultural trends – from food to art and popular fashion. Export-led models were accompanied at the outset by extensive efforts to reform education at all levels. As Gill and Kharas (2007) note, the “East Asian renaissance” was marked by an accelerated absorption of knowledge. Internationalization also increased women’s expanded participation in the work force, female literacy, and financial independence. Furthermore, the economic dynamism of these models succeeded in reversing brain drains.

Summing up, some of the explanations for competing models of political survival proposed in this chapter are more plausible than others; some are more easily subjected to empirical verification than others; some build on more solid theories than others; some are richer in causal mechanisms than others; some are particularly restricted by scope conditions, other less so; some are based on principles of methodological individualism, others on structural and cultural premises; some endogenize and contextualize more explicitly than others; some are more unpacked, others more “just-so”; some aim at middle-range theorizing, others at more universal generalizations; some are more flawed than others but, as Zuckerman’s chapter reminds us, all are bound to be flawed in a discipline bereft of covering laws or uncontested theoretical principles, let alone predictive power (Tetlock 2005).

This overview is thus only suggestive, certainly not exhaustive, offered as a first salvo in a comparative politics research agenda – particularly one focused on domestic–international linkages – aimed at untangling the sources of difference between the two regions. Running the paradigmatic gamut, they capture different facets of diverging trajectories in the domestic politics of these two regions that bear on the distinctive outcomes observed today. Comparative work along these lines can be advanced by a better understanding of the micro-foundations, as well as the institutional and cultural constraints on leaders and on their strategic interactions with domestic and international actors. This research agenda can be enriched by studies in the tradition of comparative historical analysis, process-tracing, or analytic narratives (Katznelson this volume; Mahoney and Rueschemeyer 2002); field, ethnographic, and archival research (Ross this volume); and database collection, statistical analysis, formal theory, and counterfactual experiments (Zuckerman this volume; Levi this volume). It can lean on “individualizing comparisons,” “encompassing comparative strategies,” “variation-finding strategies,” or “universalizing comparisons” (Tilly 1984). This agenda can, in short, mirror the conceptual and methodological diversity that comparative politics has exhibited in the past decade.

CONCLUSIONS

This chapter has surveyed a sample of the most recent literature on domestic–international linkages in the study of comparative politics. This literature has a much older pedigree, raising the question of how much more we know today about these linkages than a decade ago, when this book’s first edition saw light. If the convention that scientific knowledge doubles every five years were to apply here, our knowledge of the themes in this book should be severalfold what was available in 1997. Since there was no chapter on this topic in the earlier volume, there is no formal benchmark to relate to, and, in any case, leaps in knowledge (not only in the social sciences) are hard to measure with any precision.

Nonetheless, this overview suggests considerable progress in the study of domestic–international linkages. Enduring themes – wars make states (Herz 1957; Hintze 1975; Tilly 1985), global markets and embedded liberalism (Polanyi 1944; Ruggie 1982) – have been updated and in some cases significantly revived. Kasza (1996) found very sparse attention to the study of war in comparative politics syllabi in the early 1990s, but as this review suggests, inside-out and outside-in links between war and domestic politics are gaining increased attention. Newer themes – human rights, civil war, gender, environmental and other social movements, audience costs – have become more prominent as well. The artificial dichotomy between domestic and international anarchy has loosened, bringing international and comparative politics closer together (Milner 1998). Furthermore, relatively simplistic understandings of domestic–international linkages have given way to far more nuanced and sophisticated explanations, backed by various forms of evidence. Neither purely structural nor methodological-individualist reductionisms have become modal forms of analysis. More studies are avoiding Procrustean temptations to reduce politics to rigid paradigms. The Occam’s razor (*lex parsimoniae*) approach to globalization – common in the 1990s – is being deposed by complexity. There is growing realization that “actors derive their preferences from a pattern of influences involving other actors and the internal politics of other countries. Institutions, interests, and ideas within countries are constantly influencing the same factors in other countries” (Gourevitch 2002: 310). The advantages of theoretical frugality in pure paradigmatic research seem to be progressively exchanged for those of completeness and empirical validity. Methodological diversity has broadened rather than narrowed the gates to multi-method approaches to theorizing and evidence.⁵⁵ Diversity appears auspicious for political science more generally, taking into consideration recent findings by Tetlock (2005).

It is often believed that greater creativity accrues from working at the interstices of different disciplines or subfields. And so it is with the study of linkages connecting comparative and international politics, and with domestic conversion processes taking place in a global context. If we didn’t know it earlier (or chose to ignore it), it is quite clear now that these two dimensions of politics have

⁵⁵ Laitin (2002); Bueno de Mesquita (2002); Monroe (2005); Greif (2006).

always been inextricably linked, but also that we have entered a brave new world where the study of domestic and international politics in isolation makes less sense than ever before. The realities of internationalization and globalization – and challenges to them – have transformed earlier literatures on the international sources and consequences of states, revolutions, civil and interstate war, economic and political reform, terrorism and other violence, social movements, political parties, military organizations, and politicians' incentives and choice constraints. Cross-regional differences in these categories have spawned new research agendas, such as the one outlined in the section comparing East Asia and the Middle East. Leaders in these two regions read the opportunities and opportunity costs of globalization differently, given different institutional and cultural contexts. Nor has the ferocious dragon of "globalization" – to paraphrase Iversen (1996) – devoured welfare states, tax systems, party systems, local cultures and institutions, transforming all states into isomorphic markets. At least at this stage, globalization resembles more of an iguana, capable of mimetically adapting its colors to domestic conditions. The ratio of light to heat evoked by globalization has arguably changed in a favorable direction.⁵⁶ Yet, continued debates over concepts and findings drive home the fact that we are still under the fog of globalization, facing abundant uncertainty about the *long durée*. All this promises exciting times. And if the trend of the past decade continues, we may even be on a better path to overcome Galton's problem – and others – in the study of comparative politics.

⁵⁶ Iversen (1996) deplored the state of the globalization debate circa the mid-1990s as shedding "more heat than light." A decade later, greater ken has in some measure reversed that balance.

Comparative Perspectives on Contentious Politics

Doug McAdam, Sidney Tarrow, and Charles Tilly

In the mid-1990s, the study of nonroutine, or contentious, politics had become a thriving but fragmented interdisciplinary field of study, with expertise divided across a confusing patchwork of disciplinary boundaries, geographic areas, historical eras, and nominally different types of contention (revolutions, peasant rebellions, strikes, social movements, civil wars, etc.). Most of the work in the field focused on (usually reformist) social movements in the countries of the North, with distinct bodies of research on revolutions, civil wars, and terrorism and on the contentious politics of postauthoritarian transitions. In this sense, the field of contentious politics was less a consensual than an imagined terrain of study.

The field is still fragmented, disconnected, and contentious, but we see some windows of clarity in what was an obscure wall of teaching, research, and controversy a decade ago.

- First, many in the field have taken a “culturalist turn” in a deliberate departure from the dominant structuralist tradition in the social movement field, while still others have been inspired by the rational choice tradition.
- Second, changes in the real world have turned many scholars’ attention beyond the reformist social movements that were the stock in trade of the field to the study of terrorism, civil wars, and insurgencies.
- Third, in part in response to these real-world changes and in part in reaction to the fragmentation of the field, a broad perspective focusing on contentious politics has come to occupy a distinct space in the field.

In this largely rewritten chapter, our principal goal is to encourage a crossing of the various boundaries – disciplinary, historical, geographic, and between different forms of contention – that divide the field of contentious politics. A second goal is to persuade our readers that different forms of contention – like social movements and civil wars – dovetail with different types of regime. And a

This chapter was drafted prior to the tragic death of Charles Tilly in April 2008. McAdam and Tarrow wish to dedicate it to the memory of our friend, collaborator, and mentor.

third is to argue that a fruitful way of comparing these forms is to construct analytic narratives of episodes of contention and to break them down into the mechanisms and processes that drive them and connect them to their origins and outcomes.

This chapter takes up six topics. The first part, “Common Properties of Contentious Politics,” lays out our general perspective on contentious politics. That perspective, we hasten to add, has not become consensual in the field (*Mobilization* 2003). The second part, “The Evolution of the Field,” offers an updated sketch of how the field has evolved from the 1960s through the 1990s. The third part, “Mechanisms and Processes of Contention,” presents some of the mechanisms and processes that we have found central to the dynamics of contention. The fourth part, “Two Distinctive Forms of Contentious Politics,” focuses on some distinctive features of two very different forms of contention – social movements and civil wars – and their relationship to different types of regime. The fifth part, “Contentious Politics and Comparative Politics,” illustrates ways in which integrated approaches to contentious politics by a number of other scholars are contributing to comparative politics. We close by offering our thoughts on what remains to be done to create an integrated field of contentious politics. We do not deal with substantive debates, except where these mark theoretical or methodological departures in the field.

COMMON PROPERTIES OF CONTENTIOUS POLITICS

Contentious politics consists of public, collective making of consequential claims by connected clusters of persons on other clusters of persons or on major political actors when at least one government is a claimant, an object of claims, or a third party to the claims. Thus defined, contentious politics is a causally coherent domain with distinctive properties. It is causally coherent in the sense that similar cause–effect relationships apply throughout the domain. It is distinctive in the sense that some features of contentious politics appear nowhere else in social life.

Interactions, Claims, and Governments

Let us begin with common properties that are true by definition: interactions, collective claims, and governments. Contentious politics necessarily involves *interactions* among clusters of persons, the minimum set being one claimant, one object of claims, and a third party to the claims. Interaction matters because it builds on, establishes, and transforms relations among political actors. Models of political action that take up just one actor at a time cannot explain such interactions or their outcomes.

Contentious politics centers on consequential collective *claims*: calls for action on the part of some object that would, if realized, affect that object’s interests. Claims range from decorous collective expressions of support to devastating attacks. The inclusion of claims in the definition rules out

inadvertent, indirect, and incremental interactions, however politically consequential. It rules out, for example, gradual encroachment of one peasant community on the land of an adjacent peasant community. If, on the other hand, one community makes public, collective demands for some portion of its neighbor's land, the two communities move definitively into the zone of contentious politics.

Governments figure in all of contentious politics, although frequently as third parties rather than initiators or objects of claims. The involvement of governments means that contentious politics necessarily interacts with noncontentious political processes such as routine public administration, organization of elections, military conscription, tax collection, appointment of officials, and disbursement of funds. Collective contention often occurs around each of these routine political processes. But contention in adjacent arenas regularly affects them as well, for example when street disorders disrupt the delivery of governmental services. The availability of governmental coercion gives an edge to political contention that rarely exists outside of the political arena.

In political contention, most forms of contention are either conventional or confrontational – more rarely, violent. Conventional and confrontational forms of action are most typical of what we call the “social movement repertoire.” Large-scale violence always remains a possibility, however faint. Contention connected to governments does resemble contention in families, sports, churches, and businesses in some regards. But we single out government-connected contention because it has distinctive properties.

Proximate Effects

Each of the three main components of contentious politics – interactions, claims, and governments – has proximate effects that are not simply true by definition. Interaction between actors does not simply use or reproduce the relation between them. Incrementally or explosively, it transforms both actors and relations. That is true of third parties as well as of initiators and objects of claims. At a minimum, contentious interaction between A and B provides C with information about the individual propensities and capacities of A and B, about potential future interactions between them, and about likely effects of C's future intervention in political situations involving A and B.

Collective claims likewise have political effects beyond the immediate outcomes of their calls for action. Compared with those outcomes, they provide information about the future feasibility of similar claims. They lay down a publicly accessible history of interactions among the parties: X makes claims on Y, but Z then smashes both of them. Successive claims between the same pair of actors and outcomes of those claims thus create cultural material that remains available for later interaction. We can call those materials “collective memory” so long as we heed the warnings that (a) groups do not store traces of the past in the same ways that individual brains do and (b) different individuals in a group call up distinct and even conflicting memories of past contention.

Governments alter their organizations, personnel, policies, and practices in response to their participation in contentious politics. But their organizations, personnel, policies, and practices also profoundly shape contentious politics. To take the obvious point, the means of coercion – armies, paramilitaries, police, jails, secret operatives, and so on – available to a given government significantly affect its capacity and propensity to intervene in contention involving nongovernmental actors.

But regularized relations between governments and major political actors matter just as much. We can conveniently call those regularized relations “regimes.” Until we take regimes into account – as we will do in the section entitled “Two Distinctive Forms of Contentious Politics” – we cannot tell whether a government has high or low capacity, operates democratically or undemocratically, depends heavily on patronage and corruption, or excludes major segments of the population from its system of rule. Forms of governments and regimes strongly limit the character of contentious politics. Students of comparative politics regularly pay attention to the proximate effects of interactions, claims, and governments on contention.

Beyond common properties of contentious politics that hold true by definition and the proximate effects of the definitional elements, we enter a zone of adventure and controversy. For the moment, students of the subject have not agreed on a set of empirical generalizations and explanations that cut across the many varieties of contention. To be sure, some relatively general *aspects* of contentious politics have long stimulated cumulative knowledge. Some scholars investigate common properties of repression across a wide variety of contention, others study effects of democratization on contention at large, and still others analyze the impact of different sorts of social cleavages on the character and intensity of a regime’s contentious politics. Indeed, just such cross-cutting efforts give us some warrant for believing that a more thorough search for cause–effect relations spanning multiple forms of contention will be fruitful. We will turn to them in the section on “Contentious Politics and Comparative Politics.”

Many students of contentious politics have identified a number of properties that hold across a wide variety of contentious politics. Let us group some of the most important under three headings: (1) political opportunity structures, (2) collective actors, and (3) performances and repertoires.

Political Opportunity Structures. Political opportunity structures are features of regimes that affect the likely outcomes of actors’ possible claims. A largely consensual list of those features would have to include

- a. the multiplicity of independent centers of power within the regime
- b. the regime’s openness to new actors
- c. instability of current political alignments
- d. availability of influential allies or supporters for challengers
- e. the extent to which the regime represses or facilitates collective claim making
- f. decisive changes in items a to e

The higher the value of any item in a given regime, all other things equal, the greater the likelihood that actors' claims will be realized. This means, of course, that within the gross variation from low-opportunity to high-opportunity regimes, political opportunity structure also varies from actor to actor and situation to situation. Not all potential actors, for example, enjoy the same access to allies and supporters.

Of course, structures of opportunity and threat are objective factors that may or may not be perceived by challengers. In our work on *Dynamics of Contention* (hereinafter *DOC*) we have emphasized the *attribution of opportunity* as the first step in the process of mobilization (McAdam et al. 2001: ch. 2). The same is true of threat: The fact that citizens of the Soviet bloc perceived their regimes as all-powerful constrained them from rebelling until a combination of domestic and international factors revealed the rot at the core of their systems. It was as a result of this collective perception of increased opportunity and reduced threat that cascades of contentious politics began to unfurl in the decaying state socialist bloc (Beissinger 2003).

Formation of Collective Actors. In contrast to structural language that imputes durable propensities and capacities to groups and categories such as nongovernmental organizations and ethnic clusters, since the work of Alberto Melucci (1988) students of contentious politics have observed that the constitution of claim-making actors is a contingent, dynamic process. Participants frequently shift their collective definitions of who "we" and "they" are. They do so especially through two processes. First, they create new connections among individuals, networks, and previously constituted actors in the form of named coalitions, fronts, and organizations. Second, they activate, deactivate, and redraw boundaries separating one actor from another, creating collective stories about the two sides (Tilly and Tarrow 2006: ch. 3). Collective identity formation, in other words, is *relational*.

Performances and Repertoires. Instead of varying infinitely as a function of interests, opportunities, and strategic calculations, within any particular regime collective claim-making clusters in a remarkably small number of performances. In contemporary democracies, for example, many political actors know how to petition and to hold a public meeting, but not how to organize a coup d' état or assassinate a rival candidate. In many a troubled regime, however, the opposite is true: The claim-making performances of petitioning and meeting publicly are simply unavailable, while coups or assassinations are standard ways of doing political business.

Performances, furthermore, clump into sets that students of contentious politics call "repertoires." Repertoires belong to regimes and, more precisely, to recurrent claimant-object pairs within regimes. Within the democratic and semidemocratic regimes that host social movements, for example, groups of citizens that make claims on authorities can choose among petitions, public meetings, press statements, demonstrations, lobbying, and a number of other

performances that began clustering together in European and North American social movements during the early nineteenth century, and eventually spread across much of the democratic and semidemocratic world.

Existing repertoires channel contentious politics by producing widely recognizable and practicable forms of coordination and signaling. They also accumulate information about the political opportunity structure: The recent record of a particular performance tells potential claimants and objects of claims about the likely outcomes of different strategies, the likely opponents and coalition patterns, and the feasibility of coordinated action. For that very reason, governments and other major political actors regularly exert control over various known performances, attempting to prescribe some, tolerate others, and forbid still others.

Three interacting clusters of causes produce change and variation in prevailing repertoires. First, variations and alterations in everyday social organization – down to such details as the availability or unavailability of telephones and public transportation – affect the feasibility of different kinds of performances. Second, the variable and changing character of regimes impinges on political opportunity structure; on the nature of identity, standing, and program claims; and on the array of political actors actually or potentially available for contention. Third, within contention itself, improvisation and learning occur: Actors innovate, some innovations catch on, performances stop producing their desired effects, and shared understandings concerning the meanings of different forms of contention evolve. But once improvisation and learning occur in one site, they often spread to other sites through diffusion, emulation, and organized initiation, intersecting with these sites and producing distinctive outcomes.

We have framed these generalizations in language that emerged from our own work – individual and collective – on contentious politics. However, we should also emphasize that they emerged from a collective conversation among a large number of specialists in sociology and political science from the United States, Western Europe, and, to a lesser extent, countries in the South. In the next section, and at the risk of leaving out some important subtleties and disagreements, we trace how this conversation evolved and how three strands of theory and research contributed to it:

- the “political process” approach that began in the study of American politics (Eisinger 1973) and diffused to Western Europe and elsewhere;
- the collective action perspective that arose out of microeconomics (Olson 1965) and triggered a “resource mobilization” response in the study of social movements;
- and the “framing” perspective that came to the study of contentious politics from social psychology, and especially from the contributions of Erving Goffman (1974).

Among them, these three perspectives contributed to a broad consensus on the kinds of variables that are considered important in predicting the emergence and outcomes of social movements. Less consensus exists, however, on the dynamics

of contention, which we will argue can only be understood with greater attention to specifying the mechanisms and processes that drive it.

THE EVOLUTION OF THE FIELD

Perhaps more than in any other sector of comparative politics, the study of contention is highly sensitive to developments in the real world. The starting point was the Western social movement cycle of the 1960s, starting with civil rights and the New Left in the United States, broadening into the movements of 1968 in Europe, which, in turn, expanded into the “new” social movements of the 1970s and 1980s.

Three main scholarly approaches developed in response to these changes:

- a structurally rooted political process model;
- a rational choice perspective and its related resource mobilization variant;
- and a constructivist approach that draws, first, on an older “collective behavior” approach and, second, on the more general cultural turn in the social sciences.

A rapid look at the evolution of these main schools of thought, and at one representative exponent of each, will show how these approaches developed and how they differ. They will also help us to outline the foundations for the synthesis we hope to advance: *the integrated study of contentious politics, by building analytic narratives of episodes of contention based on a mechanism-and-process approach.*

The Political Process Approach

Led by the precocious rise of contentious politics in their country in the early 1960s, American scholars were the first to develop a political-structural approach to movements centering on several versions of the concept that has come to be known as “political opportunity structure.” American scholars like Eisinger (1973), the Tillys (1975), Piven and Cloward (1977), McAdam (1982 [1999]), Tarrow (1989), and, most recently, Amenta (2006) saw collective action as both a response to and an influence on institutional politics. (Western European scholars, starting from a different empirical base, developed a different kind of structuralism more influenced by post-Marxism.)

While the student movements of the late 1960s led some Europeans to infer a new class basis for social movements (Touraine 1971), others were reframing structuralism around the study of objective life-chance coalitions (Habermas 1981; Offe 1985). This approach eventually congealed into what was called in the 1980s the “new social movement” approach. There was eventually a convergence: Spurred on by an increasingly dense network of contacts across the Atlantic, American scholars soon became more aware of the European approaches, while the Europeans – despite their distaste for American inductivism – eventually focused on political institutions (compare Offe 1985 with Offe 1990; also see Kitschelt 1986; Kriesi et al. 1992, 1995).

This intersection between the European new social movement approach and the American political process approach diffused the closest thing that the field of contentious politics has to a core methodology – protest event analysis (Rucht, Koopmans, and Neidhardt 1999). Practitioners applied this methodology to episodes of contention as different as those in Britain in the late eighteenth and early nineteenth centuries (Tilly 1995b), American ethnic conflicts in the late nineteenth and early twentieth centuries (Olzak 1992), Germany from the late 1940s to the 1990s (Rucht and Ohlemacher 1992), the American civil rights movement (McAdam 1982 [1999]), Italy in the 1960s (Tarrow 1989), four other European democracies in the 1980s (Kriesi et al. 1992, 1995), and nationalist mobilization in the collapsing Soviet Union (Beissinger 2002).

Meanwhile, the study of revolutions was progressing along a parallel track with a heavy dose of the structuralism that was popular in the 1960s. In the deep historical investigations of Moore (1966), the anthropological-historical case studies of a Wolf (1969), and the systematic agrarian comparisons of Paige (1975), both Western and non-Western revolutions were examined in relation to their social-structural underpinnings. More politically oriented structural analyses of Third World revolutions were being carried out at the same time by Joel Migdal (1974). Structuralism reached its culmination with respect to revolutions in the work of Theda Skocpol.

Skocpol on Revolutions. Part of the enduring popularity of Skocpol's work on revolutions is due to the fact that it combined macrosociological and political structuralism (1979, 1994). In explaining the origins of three great social revolutions, Skocpol combined an emphasis on class relations (but *not* those of the urban proletariat) with an acute attention to states' fiscal crises, in interaction with their international vulnerability. When she turned to revolutionary outcomes, state structure loomed even larger in her vision, while international factors receded into the background (cf. Walt 1996). As for the revolutionary political process – the dynamic of political culture, coalition building, and leadership within revolutions – Skocpol's statism left little space for it, a lacuna that her critics were quick to notice.¹

In her later work on revolutions, Skocpol softened this elemental statism, added the relevance of urban classes and interclass coalitions to the overworked peasantry, and admitted that professional revolutionaries and ideologies help to make revolutions within broad structural parameters (1982, 1994). But her work remained largely free of cross-fertilization with the *political* structuralism that was developing in the social movement field; with the microhistorical emphasis on agency that was advancing the culturalist study of revolutions (Selbin 1993); with the deeper cultural approaches pioneered by Lynn Hunt (1984); with the rational choice approaches of Popkin (1979) and Taylor (1988);

¹ The corpus of criticism of Skocpol's book would fill a review article on its own. For her encyclopedic and sometimes acerbic reply to some of her critics, see her *Social Revolutions in the Modern World* (1994: Conclusion).

or with the ambitious syncretism of Goldstone (1991), Wickham-Crowley (1992), and Goodwin (1994, 2001).

Rational Choice and Resource Mobilization

In the mid-1960s, a second perspective entered the study of contentious politics from a surprising direction – economics – and particularly the version of collective action theory developed by American economist Mancur Olson (1965). Olson's work led rationalist-oriented political scientists to focus on the microfoundations of collective action and to turn from specific forms of contention to develop a general covering law intended to cover *all* forms of collective action based loosely on the central theorem of marginal utility. Olson's work electrified both sociological and political science students of movements because it questioned what existing schools of thought had taken for granted: that people will always act on behalf of claims that they "should" be making. In sociology, the school of "collective behavior" and, in political science, "relative deprivation" both assumed that grievances would produce collective action.

Best synthesized in the theoretical work of Neil Smelser (1962), the collective behavior school had emphasized the cognitive and emotional elements in collective action. Linked thematically to Durkheim's interest in "anomie," its more extreme manifestations invited caricature by regarding collective action mainly as the result of alienation and psychological disorder (Hoffer 1951; Kornhauser 1959). But even more balanced proponents of the approach never solved the puzzle that *there is no one-to-one relationship between the extent of people's grievances and their capacity and willingness to advance their claims* (McAdam 1982 [1999]). In political science, a cognate concept was "relative deprivation," which Gurr (1970) saw as the major factor that leads people to engage in contentious and violent behavior. Relative deprivation soon turned out to be a difficult construct to operationalize with the kinds of aggregate data sources that Gurr employed, but his method – minus its social psychological goal – was eventually applied to "conflict studies" of all kinds.

Olson's contributions might have gone unnoticed during earlier periods, when it was often assumed that grievances are sufficient to explain collective action. But in the 1960s, his work converged with the growing conviction of social movement scholars that grievances alone cannot explain mobilization. Indeed, Olson argued that rational people, guided by individual interest, might very well *avoid* taking action when they see that others are willing to act on their behalf. From then on, scholars of social movements would be obliged to wrestle with the puzzle of the free rider problem. In the hands of Mark Lichbach, it was specified as the "rebel's dilemma" (Lichbach 1995). Solving it became the central puzzle of Dennis Chong's work on the civil rights movement (1991).

Olson's integration into the study of contentious politics was slow and uneven. This was in part because, during a decade in which contentious politics was buzzing and blooming, he wanted to explain why collective action is *unlikely*! Empirically grounded economists, like Albert Hirschman (1982b), were quick to

point to this paradox. Moreover, Olson seemed to limit the motivations for collective action to material incentives, ignoring the thousands of people who were striking, marching, rioting, and demonstrating in the 1960s on behalf of interests other than their own. Finally, although he named his theory “collective action,” Olson had little to describe beyond *individual* motivations and the problem of their aggregation. He gave little attention either to the historical traditions and institutional contexts of episodes of collective action or to the interactions among actors, their opponents, their allies, and significant others. How could rational choice be reconciled with the buzzing and blooming movement cycle of the 1960s? Two sociologists and a social psychologist proposed answers to this puzzle.

McCarthy and Zald Focus on Social Movement Organizations. An answer to Olson’s paradox was proposed by sociologists John McCarthy and Mayer Zald. They took off from the empirical changes they saw in American society in the 1960s and 1970s – already an advance on Olson’s ahistorical deductions (McCarthy and Zald 1973). McCarthy and Zald focused on the resources available to collective actors in advanced industrial societies (1977). They agreed with Olson that the collective action problem was real, but they went on to argue that the expanded personal resources, professionalization, and financial support available to citizens in these societies provided them with an answer to the dilemma – *professional movement organizations*.² The collective action problem was real, but so were the solutions to it that social movement organizations were designed to enact.

This emphasis on organizational means to solve the collective action problem was a disappointment to European critics looking for explanations of the ultimate origins of whole movements, but it lent a refreshing concreteness to the study of movements (Zald and McCarthy 1987) and led to two decades of productive work on the organizational aspects of social movements (Klandermans 1989; Davis et al. 2005). It also produced a cottage industry of criticisms of resource mobilization. For one thing, McCarthy and Zald had adopted the cold language of economics (e.g., they wrote of movement “entrepreneurs,” “movement industries,” movement “sectors”), repelling scholar/activists who were hot from the movements of the 1960s. Where, their critics asked, were ideology, commitment, values? For another, they posited organization as a precondition for mobilization and ignored the self-production of grassroots organizations in the process of mobilization. Particularly as grassroots movements began to arise around the environment, the bomb, and gender and sexual choice, an alternative model, emphasizing informal participation and internal democracy, arose (Evans and Boyte 1986; Fantasia 1988; Rosenthal and Schwartz 1989).

A major lacuna in early resource mobilization was that, in their shift toward organization and away from grievances, McCarthy and Zald deflected attention

² Zald’s dissertation and first book (1970), unsurprisingly, dealt with the formation, transformation, and politics of the YMCA.

from the micromobilization of individuals. Dutch social psychologist Bert Klandermans tried to specify the *process* through which individuals come to participate in social movement activities (1984). He saw a funnel of causation in which movement entrepreneurs look for support within a broad but inert protest potential; narrow it through the stages of “action mobilization” and “consensus mobilization,” and mount collective action among a subset of those potential participants who were originally targeted (1989). Klandermans’s emphasis on the social construction of protest (1992) converged with the newly developing focus on the “framing” of collective action and with the study of movement discourse (Gamson 1988). In their disillusionment with resource mobilization’s utilitarian emphasis, some scholars found a paradigmatic alternative in the cultural and constructivist approaches that began to diffuse in the social sciences in the 1980s.

The Construction of Contention

As in the study of international relations, the constructivist school in the study of contentious politics grew out of a paradigm shift away from structural models. The forerunner was Italian sociologist Alberto Melucci (1980, 1985, 1988). Melucci was among the first to see social movements not as unitary actors but as centers of collective negotiation of collective identities (1989). He also saw movements as carriers of new cultural codes. This was classical constructivism, but Melucci’s emphasis on negotiation also led to more rigorous attention to social networks, best reflected in the work of his student, Mario Diani (1995). Constructivism also grew out of the isolated but inspiring work of Murray Edelman, whose 1971 book, *Politics as Symbolic Action*, was an early call to a more cultural approach to the study of contentious politics.

Constructivism first influenced students of the identity movements that developed out of the 1960s – especially the women’s and gay and lesbian movements (Rupp and Taylor 1987). These movements, and those who studied them, focused heavily on the importance of naming and on the development of new identities through practice. In a parallel development, constructivism was being applied at the same time to nationalism, where the idea of “imagining” nations was famously diffused by Benedict Anderson’s *Imagined Communities* (1990). Eventually, some scholars concluded that *all* movements construct meanings and began to see meaning construction as a movement’s primary function (Eyerman and Jamison 1991).

The social-psychological and the cultural strands of constructivism came together in the work of David Snow and his collaborators (Snow et al. 1986; Snow and Benford 1992, 2000), who adopted and adapted Goffman’s (1974) concept of framing. These scholars also helped to stimulate the contemporary interest in the mechanisms and processes in contentious politics (see the next part, “Mechanisms and Processes of Contentious”), though they were more interested in dispositional than in either environmental or relational mechanisms. They focused on the processes through which these actors align their frames

with – or seek to transform – cultural understandings (Snow et al. 1986; Snow and Benford 2000). Frame articulation, frame alignment, frame bridging, expansion, transformation, and diffusion are the dynamic mechanisms that Snow and his collaborators developed to describe the intersection between collective action frames and general cultural understandings. As a result of Snow and his collaborators' work on the frontier of culture and social psychology, "framing work" has come to be seen as one of the major functions of social movement organizations. But framing is also done by the media (Ferree et al. 2002) and by grassroots activists who construct movements from below (Lichterhan 1996).

Framing has been absorbed into the main social movement canon (McAdam et al. 1996), a development that has made that canon less "instrumental" than some of its critics maintain (Goodwin and Jasper 2004). But culturalists do emphasize issues that are given shorter shrift in "instrumental" approaches, such as emotions (Aminzade and McAdam 2001; Goodwin et al. 2001; Polletta 2006). One abiding difference is in methodological preferences: While political process theorists tend to favor event histories and resource mobilization theorists favor organizational studies, culturalists have a natural affinity for ethnographic methods and for close analysis of texts. We can see this best in the work of political anthropologist James Scott.

Scott Takes Constructivism South. Scott was among the earliest and most influential proponents of an ethnographic approach to the study of Third World insurgency. Also influenced by his colleague, Murray Edelman (1971), in the 1970s, Scott applied E. P. Thompson's moral economy concept (1966) to subsistence peasants in Southeast Asia. Although he earned the ire of a rationalist, Samuel Popkin (1977), for this move, Scott's first book, *The Moral Economy of the Peasant* (1976), remained connected to the structuralist paradigm that still dominated the study of Third World peasantries. After all, it was the *structure* of subsistence land tenure relations that produced the culture of reciprocity between landlords and peasants that Scott called the "moral economy." By the 1980s, however, Scott had departed from structuralism altogether, deriving an ethnographic method from Geertzian interpretive theory (1973b) and focusing on individual resistance and the "hidden transcripts" on which it is based (1985).

The problem with Scott's concept was that – once conceptualized – it was hard to put empirical borders around the concept of "everyday resistance." When a subsistence peasant failed to tip his hat to a landowner, was he expressing resistance or was the sun too hot that day? And when does everyday resistance give way to more overt forms of contention? Scott was good at teasing out the prepolitical forms of resistance he found among Southeast Asian peasants, but his work provided little evidence about the mechanisms and processes that turn everyday resistance into openly contentious politics sometimes and in some places. Although his work stands out among scholars of the global South for its broad reach, among scholars of social movements, more widely used by scholars of social movements was the concept of framing, as it was put forward by Snow and his collaborators.

Searches for Synthesis

To summarize what we have argued so far: The three major paradigms around which this volume is organized all had expressions in different schools of study of contentious politics from the 1960s to the 1990s:

- The structuralist paradigm had its reflection in the *political process* model and in the Moorian/Skopolian school of revolution studies and in work on social movements in Western Europe and the United States.
- The *rationalist* paradigm was best reflected in the *resource mobilization* approach and its derivatives.
- The *cultural turn* was best reflected in the study of *framing* and of the *construction of collective identities*.

Some scholars embraced either structuralism, rationalism, or culturalism as master narratives, pushing the margins of each approach as far as it would go to demonstrate its strengths and identify its limits (Lichbach 1994, 1995). But by the mid-1990s, some scholars wearied of what one of us called “paradigm warfare” (Tarrow 2004b) and began looking across these traditions for modes of synthesis. The first to do so were John McCarthy, Mayer Zald and one of the present authors (McAdam et al. 1996), who attempted to synthesize the three approaches above in their edited volume, *Comparative Perspectives on Social Movements*. A second effort at synthesis came from two Italian scholars, Donatella della Porta and Mario Diani, who synthesized political opportunity, movement framing, and social network theory into a single model of social movements (1999). After the turn of the twenty-first century, both Aminzade and his collaborators (2001) and Snow and a group of largely sociological contributors organized an influential reader along similar synthetic lines (Snow et al. 2004).

But there were limits even to these synthetic efforts. For a start, most of them centered on the specific field of social movements, which could theoretically include a wide variety of forms of contention but, in practice, was mainly based on reformist social movements in Western Europe and the United States. They made no attempt to include the violent insurgencies and civil wars that scholars like Collier and Hoeffler (1998), Fearon and Laitin (2003), Sambanis (2004), and, most notably, Gurr and his collaborators (1993) were simultaneously exploring. Nor did they have much to say – at least at first – about nongovernmental organizations’ (NGOs) advocacy in the international arena, which was to produce a growth industry of research on “activists beyond borders” (Keck and Sikkink 1998), or about the recent explosion of terrorism that has led to a largely distinct literature of its own (Sageman 2004; Berman and Laitin 2004).

We think that much of this diverse work deserves to be integrated with the common core of social movement-based research, but we do not believe that general covering laws of the type “all collective action is aimed at producing improvements in individuals’ material situations” or the equally reductive “all collective action is aimed at constructing collective identities” is going to do the job.

Even within the specific field of social movements, individuals and groups exhibit an enormous range of motivations, and when we turn to forms of action like insurgencies, civil wars, and terrorism, that range of motives widens still further. In our view, what is needed is a synthetic approach that combines *a search for common mechanisms and processes with knowledge of specific forms of contentious politics – like social movements and civil wars – in particular contexts*. In the remainder of this chapter, we first focus on the important mechanisms and processes that we see driving different forms of contentious politics, and then survey recent work on social movements and civil wars as examples of how a synthetic approach can help to illuminate similarities and differences in different forms of contention. We close by focusing on unresolved problems in the field of contentious politics.

MECHANISMS AND PROCESSES OF CONTENTION

Beginning in the 1980s, Comparative Politics and International Relations (IR) scholars both focused on the processes that drive political change. Two groups of scholars argued that the dominant correlational logic in regression analyses and other forms of statistical study leave great holes in the relations between independent and dependent variables. IR specialist Alexander George struck the first blow against the hegemony of correlational logic with his paper on “Case Studies and Theory Development” (1979). Working with Timothy McKeown, George followed up with an important paper on organizational decision making (1985). The publication of King, Keohane, and Verba’s definitive book on causal inference applying the template of mainstream quantification to qualitative work, *Designing Social Inquiry* (1994), led to a forceful reaction among qualitatively oriented scholars, which culminated in David Brady’s and David Collier’s edited book, *Rethinking Social Inquiry*, in 2004. George’s IR-based research program expanded into comparative politics with the publication, in 2005, of his and Andrew Bennett’s *Case Studies and Theory Development in the Social Sciences* (2005).

Both the George et al. and Brady–Collier research programs focused heavily on case studies, a preference that not all researchers will share. More important for our purposes, both teams put forward a strong case for process-oriented research, not as an alternative to mainstream quantification but as its complement. We identify strongly with the George and Brady–Collier emphasis on carrying out systematic analyses of political processes. For example, we think that scholars of contentious politics will learn more from focusing on entire *episodes* of contention than from studies of single-movement organizations or even of entire movements. Episodes can be decomposed into their causal mechanisms and then reassembled into more general accounts of the processes involved. This process of disaggregation and reaggregation produces analytic narratives in which actors interact with other actors (Bates et al. 1998; McAdam et al. 2001) rather than occupying center stage of the analysis, as they do in much of the social movement literature.

However, neither Brady–Collier nor George et al. named or disaggregated political processes. We think processes can be usefully divided into their component mechanisms, which are both empirically more tractable and more easily compared than the examination of broad macroprocesses, which risk turning research into traditional historical narratives. Mechanisms are the causal links between independent and dependent variables, which we define as events that produce the same immediate effects over a wide range of circumstances. Five major mechanisms, drawn from the literature on contentious politics, illustrate the kinds of building blocks that drive episodes of contention and their outcomes.

The mechanism that we call “brokerage,” for example, is the production of a new connection between previously unconnected or weakly connected sites. Its importance for contentious politics is that it lowers the cost of communication and coordination between these sites. Social movements often employ brokerage to bring previously unconnected groups or social networks into the same campaign. Brokerage also adapts general norms or frames to local contexts, as Sally Merry found in her work on the domestication of international human rights norms (Merry 2006; also see Tarrow 2005).

Another important mechanism in contentious politics is “identity shift,” as people who previously thought of themselves in a variety of distinct social roles come together to realize a unified – if temporary – identity such as worker, victim of pollution, African American, or citizen of the world. Related to identity shift is “boundary formation,” the creation of an us–them distinction between two political actors. Identity shift and boundary formation are important mechanisms in the escalation of ethnic conflict as well as in the civil rights movement, with Martin Luther King’s call for the creation of “a new Negro” (McAdam et al. 2001: ch. 10).

“Co-optation,” the incorporation of a previously excluded political actor into some center of power, is a recurring mechanism in the history of contentious politics. It has been recognized as a central part of the dynamics of contention since Michels’s work on *Political Parties* (1962). For example, it was traced by Piven and Cloward in their study of *Poor People’s Movements* (1977). Related to co-optation is the broader process of “institutionalization”: the routinization of forms of collective action and of the groups that participate in it (Zald and Ash Garner 1987; Meyer and Tarrow 1998).

“Diffusion” is the spread of a contentious performance, issue, or interpretive frame from one site to another. Without some kind of diffusion, local episodes of contention would remain local and national ones would not spread to other countries (McAdam and Rucht 1993). This mechanism was central to Sarah Soule’s work on the antiapartheid movement in the United States (Soule 1997), as it was in Sean Chabot’s careful tracing of the spread of the Gandhian repertoire from India (2002). Recently, the advent of the Internet has given an entire new scope to diffusion and to the means by which it occurs. For example, the global justice organization ATTAC (Association pour la Taxation des Transactions pour l’Aide aux Citoyens; Association for the Taxation of Financial

Transactions for the Aid of Citizens) was able to diffuse rapidly from France across Europe partly through the availability of the new media (Kolb 2005).

Finally, “repression,” action by authorities that increases the cost – actual or potential – of an actor’s claim-making, is a continual threat to contentious politics. There is a wealth of evidence on both the correlates and the outcomes of repression in work by Francisco (1996), Rasler (1996), and Moore (1998), and in the studies collected by della Porta and Rieter on protest policing and by Charles Brockett on repression and protest cycles in the social movement tradition (della Porta and Rieter 1998; Brockett 2005). The opposite of repression, of course, is “facilitation,” which social movement scholars have developed out of Tilly’s early work (1978) into the broader concept of political opportunity structure.

Many of these mechanisms have been identified with specific forms of contention (for example, identity shift and boundary formation in ethnic movements; the co-optation of social movements and their transformation into interest groups), while others have been observed in sites that are not usually considered part of contentious politics – such as educational controversies (Binder 2002). What we wish to underscore is that many of them are not limited to particular forms of contention and can therefore be used both to trace the *dynamics of contention* and to compare *different forms of contention*. For example, the combination of repression of some groups and facilitation of others can produce the radicalization of the former and the institutionalization of the latter. And the comparison of the mechanisms that drive different forms – like social movements and civil wars – can help us to sort out the common properties of contentious politics from their differences. By way of illustration, we examine a particular cluster of mechanisms that combine in all waves of contention. In “Two Distinctive Forms of Contentious Politics” we will select two very different forms – social movements and civil wars – to show how similar mechanisms occur in different forms of contention.

Mobilization: A Cluster of Mechanisms

Mobilization is central to all episodes of contention (McAdam et al. 2001: ch.3). Many scholars have identified a sequence of linked mechanisms that are present whenever mobilization occurs: the perception of a favorable balance of opportunities and threats, the social appropriation of existing organizational sites, the framing of the episode by all participants as demanding collective action, and the adoption of particular forms of collective action. Figure 10.1 summarizes the process as we specified it in our earlier work.

Examining different episodes of contention, we found these mechanisms regularly linking together. Think of the American civil rights movement: The growing domestic importance of the black vote, combined with increased international pressure on the United States to reform its racial practices in the context of the Cold War, provided new opportunities for civil rights leaders, while the move of southern blacks to the cities reduced the threat of Jim Crow practices. Black churches offered organizational sites to appropriate groups, while Supreme

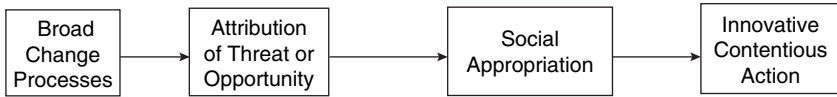


FIGURE 10.1

Court and other policy changes supported a reframing of The Problem of the South into the realization of the American rights tradition (McAdam 1982 [1999]). Other episodes of contention, like the Italian cycle of protest of the 1960s and 1970s, revealed similar combinations of mechanisms concatenating into a process of mobilization that initiated a broad cycle of contention (Tarrow 1989).

Demobilization: A Different Cluster of Mechanisms

But no process of mobilization is permanent; protest waves or cycles reverse themselves through the process of *de*-mobilization (Tarrow 1998: ch. 9; Koopmans 1993, 2004).

Demobilization is a process that is sure to follow in the wake of all waves of protest, revolutions, and strike waves. At a minimum, it appears to contain three main mechanisms: co-optation, defection, and repression. Studies of the demobilization of radical movements in Western Europe and the United States showed that demobilization simultaneously combined the co-optation of some participants into political parties and unions, the defection of others and their retreat into private life, and the adoption of repressive practices by authorities (Tilly and Tarrow 2006: ch. 5). This led to the familiar polarization between factions of a movement – some of which became institutionalized, while others either escalated into violence or retreated into sectarianism.

These generalizations do not have the status of general covering laws. The combinations of the mechanisms in the processes of mobilization and demobilization in the United States and Italy may eventually prove robust or they may not extend beyond the scope conditions of those two countries. For example, Rasler's work on the Intifada found both radicalization and intergroup competition in the Palestinian resistance, but she did not find the same relationship between conventional and confrontational actions that Tarrow identified in Italy (Rasler 2004; Tarrow 1989). Nor do we claim that these mechanisms exhaust the operative processes in contentious politics. Our goal is to stimulate students to use, challenge, modify, reduce, or expand the mechanisms we know about until their common features become clear.

Common features are more likely to occur within the same form of contentious politics, but some mechanisms and processes are common to a variety of forms. For example, revolutions and civil wars actually resemble each other; looked at closely, both include deep political polarization, mobilization of military force, armed combat, capture of crucial territories, organizations, and personnel, competing claims to represent the national interest, and appeals for

outside support. Yet, they differ precisely in the intersections among regimes, political opportunity structures, and repertoires. Still, the segmentation of the field of contentious politics into the study of social movements, strike waves, rebellions, civil wars, and revolutions has not allowed scholars to expand the use of comparative analysis beyond episodes of the same genre. In “Two Distinctive Forms of Contentious Politics,” by way of illustration, we present our view of the key features of social movements and civil wars to underscore their underlying similarities and sharp differences as forms of contentious politics.

TWO DISTINCTIVE FORMS OF CONTENTIOUS POLITICS

How do the forms of contentious politics that specialists commonly distinguish come into being and change? What distinguishes revolutions, coups, civil wars, ethnic rivalries, strikes, social movements, and other varieties of claim-making from each other? Half of our answer should already be obvious: These labels name particular conjunctions among regimes, political opportunity structures, and repertoires. Social movements and civil wars both mobilize people into action, even if the resources, the contexts, and the tools of mobilization differ. But they are bound to differ in at least some of the mechanisms and processes that drive them. Change and variation in repertoires occur in general because regimes, their political actors, and their political opportunity structures change and vary.

Social Movements

Many scholars and most activists use the term “social movements” to cover most or all of the overlap between contention and collective action, whether it is violent or nonviolent, brief or enduring, deeply cultural or overtly political. The same analysts often extend the term to the social background and cultural consequences of contention and collective action. Consider the broad use of the term “women’s movement” in the United States: It has been employed to characterize a wide range of behavioral and institutional phenomena, all the way from organized attempts to make claims against governments and other actors, to the enhanced presence of women in the workplace, to outcomes like the creation of women’s studies programs in universities and the use of feminist-fostered language among ordinary people.

Expansion of the term social movements to embrace most or all of contentious politics and their social bases and outcomes has some drawbacks: *First*, it makes systematic comparison between movements and other types of contention more difficult. *Second*, it makes it difficult to examine transitions between different forms of contention. *Third*, it obscures a fundamental historical fact: As we know them, social movements took shape only about two centuries ago and became widely available as means of popular claim-making only during the twentieth century (Tilly 2004b).

In our definition, the social movement involves sustained challenges to power holders in the name of a population living under the jurisdiction of those power

holders by means of concerted public displays of that population's worthiness, unity, numbers, and commitment. Social movements combine (1) sustained campaigns of claim-making, (2) an array of public performances including marches, rallies, demonstrations, creation of specialized associations, public meetings, public statements, petitions, letter-writing, and lobbying, and (3) repeated public displays of worthiness, unity, numbers, and commitment by such means as wearing certain colors, marching in disciplined ranks, sporting badges that advertise the cause, displaying signs, chanting slogans, and picketing public buildings. This combination produces significantly fewer violent confrontations than civil wars, rebellions, or revolutions, but it is more sustained than single protest events and less routinized than the activities of interest groups (Tilly and Tarrow 2006: ch. 6).

While revolutions occur more frequently in authoritarian regimes with relatively high-capacity governments, and civil wars break out more frequently in regimes with governments of low to medium capacity (Fearon and Laitin 2003), social movements occur more frequently in democratic or democratizing regimes in which opportunities for peaceful interaction can be easily seized and where repression is moderate. Different conjunctions of regimes, political opportunity structures, and repertoires underlie those differences. We cannot examine all the dimensions of regimes that are crucial in affecting the forms contention takes, but two such dimensions are particularly important.³ Empirically, two dimensions of change and variation in regimes play large parts in the forms of contentious politics within those regimes: governmental capacity and the extent of democracy.

Capacity and Contention. Among other characteristics, high-capacity governments exert extensive control over available means of coercion, with the consequences that governmental opponents rarely have substantial force to deploy, governmental allies can rely on forceful backing, and contentious encounters between governmental agents and opponents typically involve large disparities of force. This produces at least the constraints that permit and encourage social movements to form. Low-capacity governments, in contrast, regularly experience the formation of rival coercive centers as well as struggle *among* those coercive centers that the government itself lacks the means to suppress.

But high governmental capacity also channels a higher proportion of all contentious politics into encounters between governmental agents and others, because the higher stakes of governmental power draw political actors toward making claims on government and because the government's coercive forces regularly intervene in nongovernmental struggles. The result is that many forms of contention that pit contenders against one another in low-capacity regimes turn their attention toward the state in high-capacity systems. Although social movements typically engage in private and intergroup activities as well, high-capacity states draw their attention toward politics.

³ For one attempt to use such a framework to study the relationship between forms of regime and types of repertoire, see Tilly (2006).

From Nondemocracy to Democracy. The second major dimension that conditions the forms of contentious politics is the degree of democracy in a regime. Democratization shapes the fundamental forms of contentious politics both by creating attractive arenas for relatively peaceful varieties of contention and by promoting the formation of institutions that, in their turn, facilitate distinctive repertoires: rights of assembly, association, and speech; relatively open and accessible mass media; political parties that also operate outside of electoral contests. Social movements flourish in democracies and semi-democracies for precisely these reasons: There they find the political opportunity structures we outlined in “Common Properties of Contentious Politics” to lead them into contentious engagement with other actors and elites in the context of state institutions. Undemocratic regimes lack – and suppress – the organizational infrastructure of social movements.

The particular histories and cultures of different regimes supply the other half of our answer to the question of variation among distinctive types of contentious politics. Innovation and historical accumulation within regimes do not operate at the level of particular groups of claimants and their objects alone. Innovation and historical accumulation occur at the national and international scales as well. The examples of special-purpose associations and elections illustrate this point.

Special-Purpose Associations. Outside of elite clubs and chartered communities such as religious congregations and craft guilds, voluntary associations played no significant public part in contentious politics anywhere before the later eighteenth century. They existed in the form of regularly organized artistic, intellectual, sporting, and recreational networks. But they rarely made public, collective claims. Then associations took off, rapidly becoming one of the chief vehicles for mobilization of claim-making. Both continuously existing associations (e.g., trade unions) and associations created for the occasion or issue (e.g., many a group labeled “front,” “coalition,” “alliance,” or “citizens united for X or against Y”) became regular participants in contentious politics throughout much of the Western world. Of course, they depended on the expansion of civil liberties such as freedom of association, assembly, and speech. But their forms and practices evolved and diffused with the history of contention at the national scale.

Elections are ostensibly institutions only for structuring highly contained collective action. But in fact, as elections developed in the West, they served as focal points for special-purpose associations, networks of friends and family, and eventually trade unions and political parties that were not especially or essentially interested in electoral success. The legitimacy and legality of campaigning for elections allow both electoral and nonelectoral associations to come into the open and express their views. Elections are a clear case of the generalization of democratic institutions to nonelectoral contention. Their importance for contentious politics was illustrated between the 1990s and 2005 by the chain of antielectoral fraud movements in Eastern Europe and Central Asia (Bunce and Wolchik 2006).

Lethal Conflicts and Civil Wars

Large-scale lethal conflicts occur most often in regimes with intermediate and low levels of governmental capacity, including the unstable intermediate cases that David Laitin and James Fearon call “anocracies” (Fearon and Laitin 2003). Three forms of lethal conflict are often clustered in the literature on contentious politics and indeed overlap: *violent ethnic or religious conflicts*, *civil wars*, and *revolutions*. Much of what we say about civil wars will apply as well to these related forms, but we focus on civil wars because they have produced a sharply defined literature very different from (and largely indifferent to) the literature on social movements.

Civil wars are large-scale lethal conflicts in which the violence does not occur chiefly as a by-product of nonviolent claim-making, but forms part of claim-making’s central rationale. With civil wars, we enter a realm in which both governments and other political actors regularly use organized armed force as they make claims. Organized armed force ranges from local gangs to disciplined national armies, passing by militias, paramilitaries, private armies, and mercenaries. Sometimes organized armed force remains very one-sided, as when military units attack demonstrators or paramilitaries hunt down labor organizers. But it becomes especially lethal when at least two armed organizations battle each other.

Civil wars have special features that set them off from other forms of contentious politics. Two features in particular make a difference: the high stakes of claim-making and the problem of sustaining armed force. Killing, wounding, and damaging affect the survival of participants well after the immediate struggle has ended. They break up families and communities, destroy available labor power, and eliminate means of production. In at least one fundamental way, civil wars differ sharply from social movements: With such high stakes, potential participants in violent encounters commonly flee them unless, as participants, they are likely to prevail or to get away unscathed. But once committed, they exit less easily so long as their organization remains intact.

That brings us to the second point. Unlike recruiting people for demonstrations or public meetings, creating and maintaining armed force requires extensive resources. Some military organizations live on their own land and draw support from their own communities. But they also need weapons, ammunition, information, means of communication, and personnel to replace those they lose. Occasionally, mass killing occurs without much use of high-powered weapons. In the huge Rwandan genocide of 1994, for example, most killers slaughtered their victims with clubs, machetes, and other everyday tools. Even in that extreme case, however, the killing began with a well-trained presidential guard and militias organized by the ruling party. Reproducing a disciplined military organization depends on extensive brokerage and internal coordination. All forms of large-scale lethal conflict involve high stakes and disciplined military organizations.

Those are the differences between civil wars and social movements. Yet, we soon recognize familiar mechanisms and processes. For example, the identity mechanisms outlined in “Mechanisms and Processes of Contention” are

fundamental where people are putting their lives on the line. As in social movements, fighters are mobilized into combat, conflict diffuses, groups construct boundaries between themselves and others, and political opportunities are seized and created. As in social movements, existing political opportunity structure regularly interacts with established repertoires to shape what sorts and degrees of large-scale violence can occur within a given regime. And the overall character of a political regime (especially the capacity of its central government and its degree of democracy) strongly affects the location and the sheer possibility of large-scale lethal conflict.

States were central to the formation and interaction of social movements, and in the case of civil wars, their concentration of coercive power means that they will be involved in civil wars in one or both of two ways: as direct participants in the conflict and/or as third parties whose own power the conflict threatens. High-capacity states reduce the threat by making it difficult for anyone to create rival concentrations of coercive means within their territories, hence the predominance of conventional and confrontational – but not violent – forms of contentious politics; low-capacity states more often face precisely the threat that some rival actor will build up a major concentration of coercive means and use it to topple existing rulers.

In civil wars, two dramatic possibilities loom larger than in social movement campaigns: regime split and transfer of power. The first is that the entire regime will split, so that at least two different clusters of political actors, including agents of government, have broken their alliances and routine interactions with the others. At the extreme, two rival governments or segments of government can contend with each other, as when a rebel army establishes control over a region far from a national capital and acts like a government within that region.

Small transfers of power occur all the time in every regime: One political actor gains greater access to government, another loses access, and a third forms a new alliance with the rising actor. Competitive elections always involve some possibility of a greater realignment. Social movements position themselves within this game of marginal adjustment. But fundamental transfers of power more often accompany the large-scale violence we find in civil wars.

Of course, this thumbnail comparison of social movements and civil wars is no more than a cartoon version of how a mechanism-and-process-based comparison of different forms of contention could be framed. Delineating types of regimes and their combination of capacities and degrees of democracy would be the first stage in organizing such a comparison; examining the forms of contention that arise in these regimes would follow; analyzing the interactions between regimes and forms of contention would take us far toward the construction of a comparative political science of contentious politics.

CONTENTIOUS POLITICS AND COMPARATIVE POLITICS

Since the first edition of this reader was published, a number of scholars have been sowing seeds to advance the goal of building an integrated approach to the

study of contentious politics. A first group is attempting to test whether concepts developed in one part of the world – generally in advanced industrial democracies – apply under other scope conditions. A second group is examining transitions from one form of contention to another. A third group is extending the boundaries of the study of contentious politics into transnational space. Here we provide only the briefest examples from some of these scholars' work.

Extending Scope Conditions in China

A political process approach to contentious politics has been applied to rural China by Kevin J. O'Brien and Lianjiang Li (2006). As they gathered data on rural protest in China, it became clear to O'Brien and Li that what they "were observing did not fit snugly in the literatures on political participation, popular resistance, or social movements" (p. xii). Eschewing the area studies approaches that have dominated the study of Chinese contention, they deliberately engaged the Western contentious politics literature, and found it both revealing and wanting: revealing in that it underscored commonalities between protest in China and elsewhere, and wanting in ignoring a form of contention that they call "rightful resistance." This is the phenomenon they find when aggrieved persons employ government committees and established values to persuade concerned elites to support their claims.

One may wonder whether, as O'Brien and Li contend, rightful resistance is specific to rural China (for example, the use of the doctrine of rights by the American civil rights movement similarly used an existing frame to justify its claims), but their work has clearly profited from a deliberate engagement with recent developments in the comparative field of contentious politics. And they have posed an important challenge to conventional thinking on the relations between regimes and forms of contention: While the conventional wisdom holds that authoritarian regimes foster largely *transgressive* forms of contention, the practice of rightful resistance in authoritarian China describes an alternative pattern: the use of the law, however biased against ordinary people, to incrementally advance the boundaries of rights. Other scholars of China, though less self-conscious about their connections to the contentious politics literature, are finding similar use of dominant norms and rules in workers' use of the law in urban China (Gallagher 2006).

Transitions between Forms of Contention

One of our major complaints about the state of play in the study of contentious politics is that its segmented nature limits comparisons and isolates students of each form of contention from one another. A major problem that results from this segmentation is that it makes it difficult to examine transitions between these forms. This is particularly true of the transition from peaceful protest to violent conflict. But scholars coming from the civil war tradition are beginning to bridge this gap.

Scholars working in the conflict studies tradition founded by Gurr have produced large databases on the causes, correlates, and outcomes of civil wars and ethnic conflicts. But most of the studies using these sources are wedded to a correlational logic that fails to specify the processes that produce violence out of nonviolent forms of contention. For example, in their expansion and reanalysis of the “Minorities at Risk” dataset (Gurr et al. 1993), James Fearon and David Laitin definitively demonstrate a correlation between mountainous terrain and outbreaks of civil war (2003). But what are the *mechanisms* that are more likely to produce civil wars in (some) mountainous countries than in (most) relatively flat ones? Think of Iraq! A number of complementary or competing mechanisms could explain the correlation – social pressure to conform, topographic protection from governmental forces, the social solidarity of rural communities. Without an effort to specify and describe the connective tissue between the independent and dependent variables of interest, what could be a dynamic analysis remains largely static.

But in a series of papers and articles resulting from his work with the World Bank–sponsored project on civil wars (see Collier and Hoeffler 1998, 2000; Collier and Sambanis 2005), Nicholas Sambanis has focused on the causal mechanisms that produce civil wars out of nonviolent contention. He argues that “it is not as useful to view civil war outcomes as the result of deep-seated and hardly changing structural conditions as it is to observe the links among different forms of political violence and to analyze the dynamics of conflict escalation and the transitions from one form of violence to another” (2004: 260). Focusing on the process of escalation, Sambanis and Annalisa Zinn find that civil war is much more likely to occur if separatist conflict has already caused minor violence. They also find that separatist civil war is most likely to occur in countries in which many organizations are pursuing self-determination nonviolently. There is a clear bridge between nonviolent contentious politics over separatist aims and violent confrontations with the state (Sambanis and Zinn 2006).

Transnational Contention and Global Social Movements

Although many civil wars are exacerbated by external sponsors and international opportunities (Jenne 2006), they remain for the most part domestic in their dynamics. Not so our third example of integrative approaches – the study of transnational contention. This new wave of research promises to radically expand the range and depth of the study of contentious politics. It blends with a neighboring strand of research that developed during the 1990s – the study of “global civil society” (Florini 2003; Clark 2003). It also draws on international relations approaches – particularly on constructivism, in a field until recently dominated by realism and neorealism (Keck and Sikkink 1998). If nonstate actors can exercise “soft power” in international arenas, then states may not be as predominant in world politics as neorealists appear to believe.

Bolstered by the Internet, by cheap international transportation, and by the global diffusion of universalist cultural frames like human rights and protection

of the environment (Boli and Thomas 1999), transnational actors are becoming both more numerous in world politics, more astute in their manipulation of the international environment, and more general in their claims (Smith 2007). For example, in the 1990s, a combination of NGOs, social movements, states, and international organizations formed a powerful coalition to advance the banning of antipersonnel land mines (Cameron et al. 1998). Later in the decade, a coalition of diaspora groups, human rights advocates, and lawyers and magistrates in Europe and Latin America combined forces to indict former Chilean dictator Augusto Pinochet (Roht-Arriaza 2006). More recently, Europeans and Latin Americans have developed a robust social forum process around a series of annual global and regional social forums (della Porta 2007). Analyses of these episodes are producing evidence that challenges the predominant within-nation focus of the study of contentious politics.

Thus far, the promise of this expansion of the study of contention beyond the nation-state has been only partially realized. First, the new emphasis on nonstate actors in the international arena leaves ambiguous the enduring power of states – so dramatically evidenced in the exercise of American military expansion in the Middle East since 2001. Second, the normative commitment of many authors to “good” social movements has led them to underestimate the political calculations and political exchange relations involved in the campaigns they study (but see Bob 2005). Third, while much has been made in this literature on the interaction of the local and the global, little progress has been made in specifying the political mechanisms and processes that link subnational, national, and international actors.

Comparativist Margaret Keck and international relations specialist Kathryn Sikkink launched a program for the synthetic analysis of transnational advocacy with the publication of their study *Activists Beyond Borders* (1998). Eschewing grand and global metaphors like “global civil society” and focusing deliberately on the mechanisms in a process they called the “boomerang effect,” they described how domestic actors whose claims are blocked at home try to gain access to NGOs and other external actors to put pressure on their own governments. In the areas of human rights, on behalf of the environment, and in opposing violence against women, they showed how this process operates through loose and informal networks of activists, NGO advocates, state actors, and officials of international institutions working on common issues that cross national boundaries (chs. 3–5).

OPEN QUESTIONS IN THE STUDY OF CONTENTIOUS POLITICS

In broad relief, the field of contentious politics is still lively, still fragmented, and still more imagined than real. But scholars are thinking more broadly about the similarities and differences between movements and other forms of contention and attending more closely to the mechanisms and processes that connect them. In short, much progress has been made in moving toward a broader, more integrated study of nonroutine contention. But lots of work remains to be done.

In bringing this chapter to a close, we turn our attention to three topics that seem to us essential for the development of an integrated field of contentious politics. These are:

- the empirical features of a contentious episode,
- the relationship between “protest” and other forms of activity, and
- the methodological techniques for systematically identifying mechanisms and assessing their influence in contentious politics.

Episodes versus Discrete Forms of Conflict

The first issue has to do with our earlier argument for substituting episodes of contention for the discrete forms of conflict that have served as the traditional focal points of work in the field. In focusing only on discrete forms of challenges to constituted authority, scholars have truncated our understanding of the broader conflict dynamics that shape – and indeed, quite often, precipitate – the challenges.

How to identify episodes, however, remains a knotty conceptual and theoretical problem. Analysts face hard choices among three very different approaches: (1) try to reconstruct what participants in contention experience as a single episode, for example by taking self-reports of staged events or campaigns as units of observation; (2) adopt conventions that already appear in reporting media, for example what newspapers count as riots or police count as encounters with rioters; (3) create arbitrary but uniform units of observation, for example by regrouping available accounts into one-day segments of interaction. Each has its advocates, its advantages, and its obvious limitations. But the choice among them does not depend on common sense or convenience so much as on conflicting conceptions of what analysts are actually studying.

In the first alternative, the actors’ consciousness becomes central; analysts are often thinking of their topic as something like “protest” or “resistance.” In the second, culture and convention become more prominent; analysts are trying to locate contentious interaction within the available categories of its time-place setting. In the third, some more abstract view of contention prevails; analysts are seeking to identify common properties of contention across different forms of consciousness and various time-place settings. The catalogs of contentious events that have predominated in recent quantitative work implicitly favor the third approach, but their users often bend back their interpretations of findings toward the first or second (Franzosi 1995, 1998; McAdam 1982 [1999]; Olzak 1992; Rucht and Ohlemacher 1992; Rucht et al. 1999; Tarrow 1989; Tilly 1995b, 2004a; Tilly and Tarrow 2006: appendices).

Analysts of contentious episodes also confront a second large choice. Open contention always depends on and interacts with noncontentious social life. How much of the context, preparation, off-stage negotiation, and aftermath should enter the description of episodes as such? Students of social movements often take very broad views of their subject, including a wide range of behavior that is not, strictly speaking, contentious on the ground that it is intrinsic to

social movement action. Think of the practice of evangelical Christians “bearing witness against sin” in campaigns against alcohol and slavery in antebellum America (Young 2006); was their confession less a part of contentious politics than, say, the organizers of the underground railroad that transported escaped slaves to Canada a few decades later (Piven 2006: ch. 3)? Analysts of strikes, in contrast, typically make much sharper distinctions between strike episodes as such and what happens in work settings that generate – or, for that matter, fail to generate – strikes. Where they draw lines between episodes and contexts significantly affects the inferences they can draw about causal mechanisms and processes.

Recent innovations in the use of event analysis have made it feasible to broaden the range of events studied by students of contentious politics using the same tools as traditional contentious event analysis. Online sources broaden the range of information easily available to scholars; advances in data access and data processing facilitate inexpensive analysis, even by graduate students with limited resources; and the reconceptualization of contentious politics from protest to contention in general invites scholars to examine all forms of public events – routine and unexpected, institutional and noninstitutional around contentious issues.

In their work on immigrant collective action in Western Europe, Ruud Koopmans and Paul Statham have expanded the study of events from protest event analysis in the narrow sense to what they call “political claims analysis” (1999). They broaden the focus of their research to include all forms of the making of claims – that is, “to all actions by all actors which are relevant to our political issue field” and to deliberately coding the discursive dimensions of these actions alongside the forms of action, the number of participants, and other, more objective dimensions of the actions they study (204). In doing so, they expand the central tool of events analysis to comprehend aspects of contentious politics both within and outside of institutional politics. This takes us to some methodological considerations of a more general sort.

The Methodological Conundrum

Much less progress has been made with respect to identifying the appropriate methodology or methodologies for the study of a broad range of forms of contentious politics. The problem is much greater for students of the *dynamics* of contention than for those adopting a more conventional variable-based approach. Arguing for the causal importance of a given mechanism is one thing; systematically demonstrating it empirically is quite another. By what criteria do scholars focus on some mechanisms rather than others (*Mobilization* 2003)? How can mechanisms be observed at work? And how can their impacts be assessed?

The good news is that we see several viable strategies for documenting the empirical reality and effects of contentious mechanisms. That is what we mean to do here, at least in preliminary fashion. Specifically, we provide a brief sketch of how three very different methodological approaches can be applied to the

study of contentious politics. The three methods are (1) ethnographic fieldwork, (2) naturalistic experiments, and (3) traditional variable-based quantitative analysis of contentious events.

Ethnographic Fieldwork

As the basis for our discussion of this particular method, we turn first to Ann Mische's (2003, 2007) research on political activism among Brazilian youth organizations. Mische did extensive fieldwork in Brazil in 1994 and 1995, paying close attention to the formation and dissolution of coalitions among the broad range of organizations she studied. Much of her work focused on the "conversational mechanisms" that she felt mediated these processes. In particular, Mische identified four mechanisms as shaping the prospects for successful coalition formation. These are *identity qualifying*, *temporal cuing*, *generality shifting*, and *multiple targeting* (2003).

A second example of ethnographic fieldwork is Lesley Wood's research on the diffusion of new tactics from transnational to local sites of contention. Working in Toronto and New York, Wood studied how the "Seattle model" of ludic public performances was differentially diffused in two different opportunity structures (Wood 2004). In New York, organizers using direct action tactics readily adopted a significant part of the Seattle model, while in Toronto, protesters were less willing to embrace the new model. To examine the reasons for these variations, Wood looked at the transmitters of these new signals, the ties between them and the local receivers, and the particular contexts and organizational dynamics within the receiving sites. Wood's work shows how a mechanism – diffusion – dovetails with both the sources of a new performance and the structure of opportunities at its sites of reception.

Naturalistic Experiments

Another option open to researchers is to use the experimental method to systematically interrogate the role of particular mechanisms in contention. William Gamson, Bruce Fireman, and Steven Rytina showed the promise of this approach a quarter of a century ago in their book, *Encounters with Unjust Authority* (1982). The authors were interested in the small-group dynamics that mediate the onset of contentious collective action. They began by placing an ad in a local paper inviting individuals to participate for pay in "research involving group discussion of community standards." When each of the 33 groups of participants assembled, they were told that the discussion they were going to have would establish the community standards that would determine the guilt or innocence of a gas station owner who was being stripped of his station by a national oil company for failure to live up to the "moral turpitude" clause of his contract.

As the session unfolded, the person in charge became increasingly coercive in his efforts to elicit statements supporting the oil company's position in the case. By session's end, it was apparent that the group was being used to subvert the

criminal justice system and advance the interests of the oil company. And yet, less than half of the groups revolted in the face of this transparent injustice. Less important than their specific findings was the methodological path the authors blazed in the study. Gamson and his colleagues were actually searching for the *mechanisms* that shape the initial mobilization of contentious collective action.

Nonmainstream Quantitative Analysis

It may seem odd to include conventional quantitative analysis in the list of methodological options available to those seeking to study contentious mechanisms. After all, it was the limits of static variable analysis that helped motivate our search for mechanisms in the first place. True enough, and consistent with this position, we continue to advocate methods that allow for the direct detection of mechanisms. But we are also pragmatists who appreciate the hold that traditional variable-based quantitative analysis has on the social sciences. We are also catholic in our methodological judgments, seeing *all* methods as ingenious but, at best, crude vehicles for describing complex empirical realities. Given this premise, variable-based quantitative techniques may be among the cruder options for interrogating mechanisms, but used appropriately, they are not without value.

McAdam and Su (2002) made use of this approach in their study of the impact of Vietnam-era antiwar protests on congressional voting on war-related measures. The goal of the study was to see whether, controlling for other variables, the pace or character of the protests shaped the pace and/or valence of congressional voting. In theorizing possible effects, the authors distinguished between three possible mediating mechanisms. These are *threat*, *signaling*, and *public opinion shift*.

The first two assumed a direct relationship between protest and voting, as mediated by an inferred cognitive process on the part of lawmakers. Especially violent or otherwise *threatening* protests were hypothesized to encourage House and Senate members to vote in more movement-favorable ways in hopes of damping down public anger and restoring public order. *Signaling* assumed that especially large, nonviolent demonstrations served as a kind of mobilized public opinion, encouraging elected representatives to align their policy preferences with those of moderate protestors. Finally, *public opinion shift* hypothesized a two-step process, with protest first encouraging a decline in public support for the war, which, in turn, motivated lawmakers to align their voting behaviors with what they perceived as a general shift in the preferences of the electorate.

Unlike mainstream quantitative analysis, where the data are generally allowed to speak for themselves, this approach obligates the researchers to theorize about the dynamic mechanisms thought to account for whatever relationships they find. Secondly, researchers using different techniques may be inspired by quantitative work to design studies that more directly interrogate the mechanisms in question. This reversal of the conventional relationship between qualitative and quantitative techniques is something we very much want to encourage. Tradition has it that one employs “softer” qualitative methods to learn enough

about a phenomenon to subject it to more “rigorous” quantitative analysis. In the search for robust mechanisms in contention, reversing the order of these two approaches strikes us as more promising (Tarrow 2004a). Quantitative techniques can be used to identify – via significant statistical relationships – promising sites for more focused research using techniques better suited to the detection and measurement of mechanisms (for an example, see Rasler 2004; Migdal, this volume).

These few suggestive examples hardly exhaust the methods that could be adapted to the integrated study of contentious politics. Nor do we pretend to have done more than scratch the surface of the complex methodological issues posed by our agenda and that of others working in this field. What we hope to have conveyed by this brief discussion is the empirical viability and promise of the approach. When joined to the theoretical pluralism that animates our perspective, we remain convinced that the methodological approaches described here promise to move us well beyond the essentially static structural “facts” that currently pass for knowledge in the study of social movements and revolution.

WHERE NEXT?

We have spelled out the consequences of defining a coherent field called contentious politics: public, collective making of consequential claims by connected clusters of persons on other clusters of persons or on major political actors when at least one government is a claimant, an object of claims, or a third party to the claims. Some students of political struggle will see this chapter’s approach to that field as eclectic, while others will see it as synthetic. Certainly we have drawn freely on structuralist, rationalist, and culturalist accounts of politics without much concern for priorities among them. Yet, we claim that the analysis of contentious politics accomplishes a synthesis, however incomplete and provisional, of approaches that had previously seemed either unconnected or contradictory.

Two moves forward that synthesis. The first is to recognize that phenomena such as revolutions, social movements, nationalist mobilizations, civil wars, and democratization have common causal properties instead of each constituting an entirely separate causal domain. The second is to treat the causal properties as consisting of recurrent mechanisms and processes that in different combinations and sequences produce contrasting forms of collective claim-making, from nonviolent to violent, from routine to extraordinary, from conservative to transformative.

These two moves have strong implications for the practice of comparative politics. Instead of continuing the age-old comparative study of revolutions, social movements, nationalist mobilizations, civil wars, and democratization taken separately, they call for comparisons among the whole range of contention in different times and places. Such comparisons should take political regimes explicitly into account, drawing on the systematic theory and evidence that are accumulating on differences in contention among contrasting types of regimes.

They should also incorporate rigorous documentation of contentious episodes, preferably examined comparatively and in substantial numbers rather than considered one at a time.

We do not deny that revolutions, social movements, nationalist mobilizations, civil wars, democratization, and other forms of contentious politics have some distinctive properties. The previous presence of revolutions and social movements within a regime or within connected regimes makes available models and memories that affect subsequent iterations of revolutions and movements. To that extent, each well-defined form of struggle has a history of its own. That history deserves tracing within regimes and comparatively across regimes. But the tracing should include, precisely, a search for the mechanisms and processes by which revolutions, social movements, and other forms of contentious politics generate models and memories, not to mention the mechanisms and processes by which models and memories influence contention. More generally, our proposed program of research and theory involves a relentless effort to identify robust mechanisms and processes that recur across the full range of contentious politics.

Even more ambitiously, we cling to the hope that the rigorous comparative study of contentious politics will illuminate connections between contentious and noncontentious politics: between civil wars, let us say, and routine forms of public administration, influence-peddling, legislative maneuvering, and military logistics. It would be a very bad joke if the study of contentious politics, after challenging excessively sharp distinctions among revolutions, social movements, and other forms of struggle, fostered the illusion of a *sui generis* realm, the realm of contention. The comparative study of contention should sharpen, not dull, scholars' awareness of causal connections among the multiple forms of political interaction, contentious and noncontentious, that take place daily within every regime.

Citizenship in Democratic Politics

Density Dependence and the Micro–Macro Divide

Robert Huckfeldt

INTRODUCTION AND OVERVIEW

More than a century of systematic analysis regarding citizens and democratic politics has produced a continuing tension that centers on what has sometimes been called the “micro–macro divide” (Eulau 1963, 1986). Quite simply, should our understanding of electoral politics be focused on aggregates or individuals – on the properties, performance, and behaviors of aggregated electorates or on the choices, capacities, and opinions of individual citizens? While this question is fundamental to theoretical and methodological developments in political analysis, it is particularly compelling with respect to comparative analysis, and this chapter argues that its resolution involves a thoroughly comparative approach to the study of democratic politics.

At the most basic level, this tension involves whether analysts should employ aggregates or individuals as the basic level of analysis. The increased prominence of methodological individualism in all its various forms means that many efforts have rejected the former for the pursuit of the latter, embracing a framework built on some combination of individual preferences, utilities, attitudes, opinions, and resources. At the same time, methodological individualism comes at a price, producing a frequently cumbersome and sometimes irrelevant framework for the analysis of politics writ large. The analytic difficulties posed by methodological individualism have generated a number of theoretical and substantive shortcuts to make analyses tractable. The state as a unitary actor, the weight of public opinion, the public mood, the existence of a political culture, and the imposition of structural constraints are aggregate inventions constructed, at least in part, as explanatory devices to avoid the unwieldy and sometimes inappropriate apparatus imposed by a reliance on citizens as the ultimate component units of democratic politics.

At a different level, the answer to the question of aggregates versus individuals is often based on the very real constraints and opportunities imposed by the

utility and availability of various bodies of evidence. In a straightforward example, if one wants to explain support for the Nazis during the declining years of Weimar Germany, surveys are unavailable and aggregate evidence provides the only alternative (O'Loughlin 2002; Hamilton 1982; Brown 1982). Similarly, if one wants to study contemporary levels of racial polarization in local American elections, surveys are once again unavailable, and analyses must be based on evidence typically taken from election returns recorded on the basis of small-scale political geography (Grofman 1998; King, Rosen, and Tanner 2004). In a less obvious but substantively compelling manner, the explanatory match between appropriate levels of analysis and appropriate levels of evidence cannot be taken for granted. Individually based explanations are sometimes best evaluated on the basis of aggregate-level data (Page and Shapiro 1992; Erikson, MacKuen, and Stimson 2002; Kramer 1983), and aggregate explanations are sometimes best evaluated on the basis of individual-level data (Tingsten 1937; Huckfeldt and Sprague 1995).

A very important problem remains, however. When individual behaviors and attitudes vary systematically across aggregates, analyses will be theoretically and empirically inadequate if they are wholly undertaken either at the level of individuals or at the level of aggregates. Indeed, in situations such as these, analyses are likely to produce inconsistent, anomalous results.

A primary source of these various contextually based anomalies is anchored in the patterns of interdependence that persist among individual citizens. To paraphrase Durkheim, an aggregate composed of interdependent individuals presents a reality that is quite different from each of the individuals taken separately.¹ These patterns of interdependence among and between groups and individuals are anchored in particular places, at particular times, and characterized by particular settings. This means, in turn, that time, place, and setting are crucial to an understanding of democratic politics in general and comparative politics in particular. As a consequence of individual interdependence, groups have a reality that is separate from their constituent individuals, just as individuals have a reality that is separate from the groups to which they belong. *Hence, it is no more appropriate or inappropriate to consider groups absent individuals than it is to consider individuals absent groups.* Methodological individualists, including rationality theorists, benefit by taking account of structure and culture, just as culturalists and structuralists benefit by taking account of the role played by individuals.

The ultimate solution to the problem of the micro–macro divide is to construct explanations for behavior that take into account these patterns of interdependence among actors – explanations that provide an analytic device for moving back and forth between individuals and aggregates. In this way, the solution to the problem is thoroughly comparative. The focus is not on a comparative analysis of political systems or individuals or institutions, but rather

¹ The original quotation is taken from Durkheim (1951: 320): “the group formed by associated individuals has a reality of a different sort from each individual considered singly.”

on the patterns of relationships among citizens and institutions within political systems and the manner in which these relationships vary across political systems. Hence, the recognition of interdependence leads to a thoroughly comparative understanding of democratic politics both at the level of individuals and at the level of aggregates.

The distinctive patterns of interdependence that arise due to patterns of relationships among actors are not, of course, the only way that time, place, and setting become important for politics. An extensive menu of institutions, system characteristics, and properties produce profound and lasting consequences for politics at the levels of aggregates and individuals – proportional representation, single-member legislative districts, parliamentary versus presidential systems, federal versus unitary systems, the distinctiveness of political histories, levels of economic development, geographic variations, and much more. Indeed, system characteristics such as these also have important implications for the forms and patterns of interdependence within and among the individuals and groups that constitute the population (see Katznelson this volume).

This chapter does not pretend to provide an exhaustive treatment of all the various system characteristics that produce important consequences for the exercise of citizenship. The more modest goal is to address the implications of interdependence among individuals for the ways we understand citizenship and politics. The less modest goal is to convince the reader that these patterns of interdependence have fundamentally important consequences for cultural, structural, and rational choice theories of politics – for the ways that we understand both groups and individuals in the political process.

The chapter proceeds by addressing interdependence and levels of analysis in the study of comparative politics before addressing the relationship between ecological and individualistic fallacies in political analysis. Comparative politics provides a general case of multilevel analysis, and the data requirements for such a comparative framework are addressed. The argument turns to social networks as a key device, both for producing patterns of interdependence and for generating contextually dependent patterns of political behavior. Finally, attention turns to the implications for comparative analysis, for understanding the political capacity of individuals and aggregates, and for the various paradigmatic approaches to the study of comparative politics.

PATTERNS OF INTERDEPENDENCE IN THE STUDY OF COMPARATIVE POLITICS

Yamagishi and Yamagishi (1994) illustrate the importance of variation in patterns of interdependence for comparative analysis. In a critique of the literature on social capital, they argue that citizens of Japan and the United States demonstrate quite different levels of generalized trust – Japanese are much less likely than Americans to trust strangers. This leads, they assert, to very different patterns of relationships among and between citizens in the two countries. Due to this absence of generalized trust, the Japanese depend on patterns of recurrent

relations with well-known associates across a broad range of activities. As an example, Yamagishi and Yamagishi point to distinctive forms of economic organization among Japanese firms – particularly to the development of tightly defined networks of economic collaboration.

At the same time that patterns of social relationships and economic organization respond to variation in levels of generalized trust, it is quite likely that social and economic organization simultaneously affect underlying levels of trust. Indeed, in situations such as these, it would be quite difficult to locate the primacy of either generalized trust or the organization of social networks. Introverted patterns of recurrent relationships are likely to sustain and give rise to heightened levels of suspicion regarding those individuals who lie beyond the reach of these high-density networks. Hence, individually held attitudes and beliefs toward others give rise to patterns of interdependence within populations, just as these patterns of relationships give rise to distinctive attitudes and beliefs (see Rodden this volume).

The mistrust of strangers and the resulting reliance on networks of recurrent relations extend beyond economic matters to envelop politics as well. Kyogoku (1987: ch. 3) points to the Japanese distrust of outsiders in his development of the collectivity and battlefield paradigms, and he explains the significance of these paradigms for politics and citizenship. (Also see Richardson and Flanagan 1984.) Moreover, Japanese invest more heavily in the maintenance of these recurrent relations, with possibly unforeseen consequences. For example, Japanese are no more likely than Americans or Germans to report agreement within networks of political communication. At the same time, they *do* report lower levels of *disagreement*, and the residual level of indeterminacy in the communication of network preferences is correspondingly increased (Huckfeldt, Ikeda, and Pappi 2000, 2005; Ikeda and Huckfeldt 2001).

In these ways and many others, distinctive patterns of interdependence give rise to important political implications for the exercise of democratic citizenship and, correspondingly, for the performance of democratic political systems. Variation in these patterns of interdependence among citizens is an empirical reality of human existence that carries important political consequences, not only for voting but also for public opinion, participation, collective action, political communication, the development of political expertise and citizenship capacity, and much more. Furthermore, while interdependence among citizens is relevant to rationality, structural, and cultural views regarding comparative politics, it does not necessarily favor any one of these, and it is relevant to all three. Finally, patterns of interdependence produce consequences for levels-of-analysis problems that lie near the core of comparative political inquiry, and these problems are directly related to levels of meaning in democratic politics.

LEVELS OF MEANING IN THE STUDY OF DEMOCRATIC POLITICS

Even among those students of democratic politics committed to various forms of methodological individualism, alternative levels of analysis provide more or less

appropriate vantage points from which to understand particular political phenomena (Anderson this volume). While politics involves multiple levels of meaning (Eulau 1989), the history of modern political analysis can be written in terms of movement from the aggregate to the individual, with a more recent return to aggregates and multilevel treatments. What are the advantages of multiple levels of analysis, and how do the various levels relate to alternative traditions and approaches to the study of comparative politics?

One can readily construct stereotypical caricatures regarding various approaches to the study of citizens in democratic politics: the Columbia model, the Michigan model, the psychological model, the sociological model, the political economy model, aggregate models, and so on. Such an exercise serves a useful purpose insofar as particular analytic viewpoints and measurement devices give rise to characteristic strengths and weaknesses. At the same time, these various approaches map quite imperfectly to the cultural, structural, and rationality approaches to the study of comparative politics. Moreover, the utility of these caricatures is easily swamped whenever one encounters signal efforts that have quite clearly pushed the boundaries of their own theoretical and empirical frameworks, and it is often at these junctures that analytic progress can most clearly be seen (Tingsten 1937; Converse 1969; Jennings and Niemi 1981; Przeworski and Sprague 1986; Shively 1969; Fiorina 1981).

A primary engine driving the development of these various analytic approaches has been the availability of alternative methodological and observational technologies for studying politics. Measurement capacities have, depending on one's perspective, either increased the analytic possibilities or dictated the focus and direction of theoretical developments in understanding citizenship. Prior to the advent of the modern sample survey, political analyses of citizens and politics were limited to aggregate data regarding constituencies, states, provinces, lands, counties, wards, precincts, and so on. With the development of high-quality polls and surveys during the 1940s and 1950s, important new data collection efforts were launched – particularly the Columbia University–based studies of Elmira and Erie County (Lazarsfeld, Berelson, and Gaudet 1948; Berelson, Lazarsfeld, and McPhee 1954) and the University of Michigan–based series of national election studies (Campbell, Converse, Miller, and Stokes 1960, 1966).

The empirical possibilities for understanding citizens underwent a seismic shift as a consequence of these efforts. The availability of high-quality survey data made it possible to address problems and questions that lay previously beyond the reach of political scientists. At a distance of more than 50 years, it is perhaps difficult to recapture the excitement (and controversy) that accompanied these developments. As recently as the early 1980s, near-capacity audiences would gather at American Political Science Association convention panels organized solely to hear the report on the most recent University of Michigan–sponsored presidential election study.

The advent of the sample survey is often held responsible for the importation of a particularly individualistic form of social psychology into political analysis. According to this view, the ready availability of data *about* individuals led to an

analytic focus *on* individuals. Rather than studying institutions, structures, elites, and policies, many political scientists began focusing on the preferences, motives, and attitudes of individual citizens, thereby diverting political science from its fundamental mission. While it is certainly true that the survey data shifted the focus of many political analyses toward individual-level factors, such a criticism is an oversimplification for several reasons.

First, important work by Converse (1969), Miller (1956), Miller and Stokes (1963), Butler and Stokes (1974), and Almond and Verba (1963) adapted survey data to analyses of political systems employing multiple levels of analysis. These studies examined politics and citizens relative to larger settings – counties, nations, constituencies, and congressional districts – thereby foreshadowing later developments aimed at higher-level understandings of individual citizens. Second, in a particularly influential effort, Converse (1964) employed individual-level survey data to inaugurate a stream of research that challenged the citizenship capacity and political primacy of individual citizens. Not only did this work force a reassessment of citizens' abilities as political analysts, but it also pushed scholars to look beyond the individual citizen in constructing explanations for the performance of democratic political systems. Finally, while the ready availability of survey data led to an initially rapid development in political psychology, it also sustained a delayed but equally rapid growth in political economy. Fiorina (1981) employed rational choice assumptions and University of Michigan survey data to call into question the interpretations of citizenship offered by the collectors of the data. In short, the post–World War II revolution in political analysis was neither monolithic nor consistently hostile to explanations of politics anchored in structural, cultural, or political economic factors.

An equally influential confluence of events involved the ready availability of survey data combined with Robinson's (1950) identification of the "ecological fallacy." At the same historical moment that individual-level data were rapidly becoming available, Robinson set forth his influential argument that aggregate data were inherently suspect in the construction of individual explanations. One might argue that this combination provided a one-two punch that marked a clear shift in focus from aggregate to individual-level data, and with that shift in focus have come attendant costs and benefits.

ECOLOGICAL FALLACIES, INDIVIDUALISTIC FALLACIES, AND THE PROBLEM OF INTERDEPENDENCE

An ecological fallacy occurs when an analyst wrongly infers that an aggregate correlation between two population characteristics indicates that the characteristics are correlated at the individual level. In his famous paper, Robinson considered the aggregate and individual relationships between literacy and foreign-born status. An aggregate analysis of the 48 states produced a strong positive correlation between the literacy level and the foreign-born proportion of the population. In contrast, a parallel individual-level analysis showed a negative correlation in which foreign-born individuals tended to demonstrate

lower literacy levels. Hence, a naive analysis based on the aggregate results might have wrongly imputed higher levels of literacy to foreign-born individuals. In fact, the positive aggregate relationship was more likely to be driven by migration patterns among immigrants.

One solution to the danger of ecological fallacies, the one that many analysts adopted, was to eschew aggregate evidence in favor of individual-level evidence at every opportunity. An alternative solution was to apply more sophisticated analytic frameworks for problems that combined individual and aggregate evidence. For example, in his compelling and classic analysis, V. O. Key (1949: 5) argued that the “hard core” of the political South was made up of those counties with large African American populations, where “a real problem of politics, broadly considered, is the maintenance of control by a white minority.” In his subsequent and complementary analysis of southern support for George Wallace’s candidacy for the presidency, Wright (1976) shows that southern white voters were more likely to support Wallace if they lived in counties with higher percentages of black residents. Analyses such as these are compelling because they do not rely on simple aggregate correlations. Rather, they are built on careful analyses that take both individual and aggregate variation into account.

What would have happened if Wright and Key had instead relied on simple aggregate-level correlations? As an example, consider the simple regression of the proportions in Deep South counties voting for Wallace on the proportion of African Americans in these counties. Based on the analyses and insights of Key and Wright, we would expect a positive slope, and if we ignored the danger of the ecological fallacy, we might reach the absurd conclusion that a positive slope meant that blacks were more likely than whites to support George Wallace. This chapter’s intention is not to minimize the dangers of ecological fallacies: If the mistakes had always been so absurd, the problem would have been more readily recognized! At the same time, a modest amount of theory or substantive knowledge goes a long way in disentangling these relationships – theory or knowledge, for instance, that African Americans were unlikely to have supported Wallace in 1968. Moreover, disregarding aggregate evidence produces its own set of unfortunate consequences.

Goodman’s (1953, 1959) insights regarding these problems have provided several generations of scholars with important insights regarding the relationship between individual behavior and aggregate properties. His analyses show that the danger of ecological fallacies arises when individual behavioral probabilities vary systematically across aggregate units of interest. If behavioral probabilities are independent of aggregation units, the ecological regression provides unbiased measures of the underlying population parameters, and individual-level estimation of these parameters would provide similar unbiased results. Conversely, if behavior is density dependent and hence varies as a function of aggregate population characteristics, neither simple aggregate analyses nor simple individual analyses are likely to be adequate. This last point is important because it speaks directly to the potential for individual-level data to overcome the problems produced by ecological fallacies.

First, if aggregate-level analyses fail when individual behavioral probabilities vary systematically across the units of aggregation, what produces this variation? The probabilities vary as a consequence of (1) individual-level self-selection processes in which individuals intentionally locate themselves in distinctive social and political settings, (2) extraindividual sorting and mixing processes that produce systematic variations across aggregation units, and (3) various persuasion and communication effects, or contextual effects, arising as a consequence of population composition.

These alternatives, regardless of whether they are volitional or nonvolitional, involve patterns of interdependence among and between individuals and social groups. Thus, while the choice of a location in social structure – neighborhood, church, workplace, softball league – is often motivated by individual preference, the choice inevitably produces unforeseen consequences that are far removed from the original motivation. At least in the abstract, one might be able to take account of all the factors that lead individuals to choose particular locations in social and political space, thereby accounting for potential sources of variation in the underlying behavioral probabilities. To the extent that these choices lead to consequences extending beyond the motivation for the choice, one will not be able to explain political behavior without explicit reference to locationally specific factors.

Second, *the same factors that give rise to ecological fallacies also give rise to individualistic fallacies*. Returning to the analyses of Key and Wright and others, let us suppose that they were unable to locate white southerners within the racial ecology of southern counties. That is, suppose that they based their analyses on individual-level data without county-level identifiers. They would have found moderate levels of racial hostility among whites that would have understated racial hostility in predominantly black counties and overstated racial hostility in predominantly white counties. In summary, to the extent that individual behaviors – attitudes, preferences, choices, opinions, votes, participatory acts – are contingent on space, neither wholly individual-level nor wholly aggregate models will provide politically compelling explanations. As a consequence, excessive reliance on individual-level data has the effect of replacing ecological fallacies with individualistic fallacies.

COMPARATIVE POLITICS AS A GENERAL CASE OF MULTILEVEL ANALYSIS

The insights of Key and others address a particular problem related to race in American politics, but their work points toward a more general micro–macro problem in comparative politics. An early and revealing treatment of the problem arises in the work of Herbert Tingsten (1937) on voters in Stockholm during the early 1930s. When Tingsten plots the socialist proportion of the vote in Stockholm precincts on the working-class proportion of the precinct population, the resulting scatter follows a pronounced nonlinear s-curve pattern. At low working-class densities, the socialist parties' share of the vote falls below the

working-class proportion of the population, but the vote share exceeds the working-class proportion at high working-class densities. The nearly inescapable conclusion that arises on the basis of Tingsten's analysis is that the probability that individual workers (and perhaps individual nonworkers) supported the socialists varied as a function of the population's composition.

Tingsten's suggestive results regarding socialist voting are vulnerable to arguments regarding ecological fallacies because they are based on aggregated evidence. In a parallel analysis, however, he considers voting participation among workers based on group-specific turnout data collected and reported by the Swedish government. In this analysis, which is *not* vulnerable to the standard ecological fallacy critique, he shows that the turnout probability among workers also increases as a function of working-class densities in the precincts. In short, Tingsten's early analyses demonstrate quite clearly that the individual probabilities of turnout and socialist support vary systematically across units of aggregation. In situations such as these, neither a model based wholly on individual-level factors nor a model based wholly on aggregate-level factors would be adequate to the analytic task.

Results such as these led to a series of aggregate models, inspired by the work of Goodman (1953, 1959) and aimed at incorporating a broad range of density-dependent variation in individual-level behavioral probabilities (Przeworski and Teune 1970; Przeworski 1974; Sprague 1976; Erbring and Young 1979; Achen and Shively 1995). These efforts not only produced a rich, finely grained texture for reasoning through the theoretical implications of density dependence in citizenship behavior, they also demonstrated the observational challenges that arise in the estimation of multilevel models based on aggregate data alone. The source of the ecological inference problem is insufficient information to identify model parameters (King 1997), and this literature demonstrates the true severity of the problem. Even modest efforts to identify the source of variation in the individual behavioral probabilities within these aggregate models yield daunting identification problems. With several notable exceptions (Przeworski and Sprague 1986; Achen and Shively 1995), few solutions to the problems have appeared, and therein lies the motivation for the pursuit of individual-level explanations.

More recent efforts have pursued multilevel models that combine individual-level data regarding behavior and characteristics with contextual data for the units of aggregation where individuals are located. Rather than estimating the individual probabilities within the context of aggregate models, these efforts pursue analyses based on individual records and reports of these criterion behaviors, thereby gaining leverage on the problem of insufficient information in making cross-level inferences. This does not minimize the theoretical heuristic contribution of aggregate modeling efforts. These efforts make it clear that the goal of modeling individual political behavior, particularly in a comparative context, is to recover the density-dependent sources of variation in the individual-level behavioral probabilities – to produce a micro-model that is directly integrated within its macro context. *In this way, these multilevel models provide a direct implementation of the comparative insight in social and political analysis.*

DATA PROBLEMS AND EXEMPLARY EFFORTS

A major historical impediment to progress has been a lack of survey data that were spatially referenced with respect to politically meaningful political units. That is, the postwar explosion in survey research produced individual-level data sets that were not designed to connect individuals to contexts. Even the early Columbia studies, which were classic efforts to understand individual preferences within the contexts of particular communities, offered very little in the way of comparative analyses of alternative subcommunity settings. Thus, with several compelling exceptions (Converse 1969; Miller 1956; Putnam 1966; Miller and Stokes 1963; Butler and Stokes 1974), little progress was made in constructing analyses that located individuals and individual behavior within particular settings and contexts.

This situation began to change, primarily due to community-based studies that included subcommunity locational information on individuals. An exemplar of these developments occurs in Segal and Meyer's account of the social contexts underlying political partisanship among a sample of New England residents at two different levels of aggregation, the ward and the town. Their chapter is one among an important series of comparative studies addressing these problems in Dogan and Rokkan's (1974) edited volume, *Social Ecology*.²

While they are concerned with much higher levels of aggregation, the contributions of Almond and Verba (1963) and Verba, Nie, and Kim (1978) are particularly important in this intellectual context. Both studies demonstrate a thoroughly comparative vision regarding the exercise of citizenship. For the first time, political scientists were able to consider not only aggregate variation in patterns of democratic politics across nations, but also variations in the individually based patterns of relationships within and among nations.

Finally, another important contribution was produced by Langton and Rapoport (1975) in their analysis of support for Salvador Allende among residents of Santiago. They show that working-class residents who lived in working-class districts were more likely to identify as workers, to see Allende as favoring workers' interests, and to vote for Allende. This effort and others are particularly important because they pursue an identification of the mechanisms that produce spatial variation in patterns of individual behavior.

Many (but certainly not all) of these data limitation problems have been resolved. Numerous collections of individual-level data with geographic identifiers are now available, and the growth has been particularly pronounced with respect to cross-national data collection efforts. The Euro-barometer, the Comparative Study of Electoral Systems, and other similar efforts are producing important new data resources that combine individual, aggregate, and institutional data, most frequently at the national level (Shively and Kedar 2005). As a consequence, important progress is being made in the development of statistical models for analyzing these data sets (Peffley and Rohrschneider 2003), and

² This volume originally appeared under the title *Quantitative Ecological Analysis in the Social Sciences*.

substantively rich cross-national analyses taking account of interdependent actors have appeared (Anderson and Paskeviciute 2005).

SOCIAL NETWORKS AS THE CONNECTING TISSUE BETWEEN INDIVIDUALS AND AGGREGATES

The question that naturally arises is, why do the individual probabilities of various behaviors vary as a function of context and setting? What is the source of density dependence, and what is there about working-class neighborhoods in Santiago and Stockholm and elsewhere that alters the likelihood of supporting particular political parties? A number of explanations have been identified and considered: party activity (Huckfeldt and Sprague 1995), reference group orientations and political loyalties (Langton and Rapoport 1975), and various forms of social interaction.

This last possibility is particularly important, both because it constitutes a component of each of the other explanations for contextual effects (Eulau 1989; Huckfeldt 1986) and because it provides a means for avoiding the oversimplification involved in assumptions of random mixing. Indeed, social interaction lies at the core of nearly any resolution to the micro–macro dilemma, either as a direct or indirect source of the mechanism creating the effect. Returning to Langton and Rapoport's Allende supporters, if working-class consciousness lies at the core of the contextual influence mechanism, how do these contexts affect class consciousness? Presumably, patterns of interaction with other workers cause workers to be more or less likely to adopt a working-class identity, but are the social interaction patterns of nonworkers also affected? Do patterns of interaction generate effects beyond their effects on social loyalties and identities?

At the same time that important developments were occurring in various forms of multilevel analysis and the identification of contextual effects, another tradition was rapidly developing in the identification of social networks. This tradition has produced analytically powerful implementations of network research involving complete enumerations of the relationships within well-defined, self-contained populations (White 1970). That is, these studies document the presence or absence of relationships between all the dyads within a population, and this produces an opportunity to employ a range of powerful analytic techniques (Wasserman and Faust 1994; White, Boorman, and Breiger 1976). These analytic techniques are very useful to many political science research settings, but with several notable exceptions (see Laumann and Pappi 1976; Knoke, Pappi, Broadbent, and Tsukuba 1996), they have seen little application to the study of comparative politics. In general, fully articulated network studies are more likely to be employed successfully within substantive applications involving interactions among political elites, within policymaking systems, and within and among courts and legislatures (Heinz, Laumann, Nelson, and Salisbury 1993; Lubell and Scholz 2001; Fowler 2006). They are less applicable in the context of the large populations that provide the primary object of study for scholars concerned with studies of citizens and democratic politics.

Within the domain of elections and public opinion, contextual measures of population composition often have been treated as an alternative measurement device for networks of political communication and influence. The typically motivating assumption is, for example, that Stockholm residents are more likely to communicate with members of the working class if they live in precincts with higher concentrations of workers. The problem with this practice is that it obscures the very real differences between contexts and networks in the study of voting, elections, and public opinion (Huckfeldt and Sprague 1995). In particular, the underlying random mixing assumption is based on the problematic premise that individuals fail to exercise choice and control in the construction of their communication networks.

A breakthrough occurred when Laumann (1973) and others began to implement network name generators within traditional survey interviews. In his study of Detroit males in the 1966 Detroit Area Study, interviewers asked respondents to identify three friends and then asked the respondents a battery of questions about each of these identified individuals. Later analyses (Huckfeldt 1986) combined neighborhood-level data with the 1966 Detroit Area Study data to pursue a number of questions with respect to the relationship between contexts and networks. Are individuals more likely to have working-class friends if they reside in working-class neighborhoods? Do individuals exercise choice and control over the class composition of their networks – do they construct networks that reflect their own class membership? This work showed that, while individuals exercise preference and choice in the construction of these networks, their control is incomplete. A stochastic process of social interaction generates opportunities for association reflecting the supply of potential associates in the surrounding context, and these recurrent opportunities circumscribe individual control in network construction. Thus, while the random mixing assumption misrepresents the construction of communication networks, the ability of individuals to control the composition of even their most closely held circle of associates is limited. In short, social interaction processes leading to the development of political communication networks include a substantial stochastic component, reflecting locally determined social interaction probabilities, and in this way individuals are connected to spatial aggregates via stochastic processes of social interaction.

Other analyses confronted the implications for explicitly political behavior. The 1984 South Bend study – which addressed a national campaign in a local community – employed a panel study with a network name generator, as well as a snowball survey that included interviews with the members of the networks (Huckfeldt and Sprague 1995). Analyses of these network data reinforce and extend the conclusions based on the Detroit analysis. All else being equal, Reagan voters were more likely to talk about politics with other Reagan voters, but they were also more likely to talk about politics with Mondale voters if they were located in contexts where support for Mondale was high. Hence, the objective content of the partisan signal received through political communication networks is a complex product of both supply and demand – on the

preexistent political preferences held by individuals, as well as the distribution of preferences within an individual's surroundings.

Patterns of political interdependence among citizens are neither a simple function of aggregate population composition nor simple derivatives of individual preference. Democratic politics is not reducible to the motives and preferences of individuals, but neither can it be subsumed by reference to the aggregate characteristics of surrounding environments. Citizenship occurs at the countless intersections between and among individuals who are embedded in distinctive environments, and the resulting networks of political communication reflect both individual characteristics and the characteristics of the population surrounding the individual. In this way, networks of political communication provide an essential ingredient to the resolution of the micro–macro problem in political analysis.

Finally, organized efforts at partisan mobilization take on particular importance when they are understood within the context of citizen communication networks. When a campaign worker knocks on a door or makes a phone call, she is intervening within the existing political communication networks where citizens are typically located. These interventions are more likely to be influential when party workers are located within the same neighborhoods, workplaces, and houses of worship as the targets of the partisan contacts. While these conditions may be less likely to hold in the modern era, various efforts have documented the continuing effects of personal contacts as agents of political mobilization (Gerber and Green 2000; Nickerson 2006; Huckfeldt and Sprague 1995; Rosenstone and Hansen 2003). As Powell's (1986) comparative work on voter turnout has demonstrated, vigorous organizations of mass mobilization provide a key factor in explaining higher levels of voter participation, and these efforts at mass mobilization are most likely to be effective when they are able to penetrate the existing communication networks of citizens.

IMPLICATIONS FOR COMPARATIVE ANALYSIS

What are the implications for comparative analyses in general and for the micro–macro divide in particular? The earliest work on contexts and networks focused on very-small-scale spatial analyses – on the implications for neighborhood population composition for the signals transmitted through political communication networks. While this points toward one end of the spectrum for a thoroughly comparative analysis of democratic politics, it is far removed from the opposite end of that spectrum – the relationship between democratic politics and the distribution of preferences and opinions within populations defined on the basis of nations, states, and regions. This chapter's argument is that the same principles come into play at both ends of this analytic spectrum. That is, distributions of opinions and democratic values are important at both small-scale and large-scale levels of aggregation.

The importance of national-level opinion distributions for comparative analyses is demonstrated in a cross-national study (Huckfeldt, Ikeda, and Pappi 2005) based on the first pan-German election in East and West Germany in

1990, the U.S. presidential election in 1992, and the Japanese parliamentary election in 1993. This analysis of disagreement patterns across parties and across nations suggests that aggregate distributions of preferences generate consequences for individual networks of communication, just as the individual processes of communication carry important political implications for the national parties and national politics. Partisan preferences are clustered within communication networks – citizens are more likely to discuss politics with like-minded discussants than a random mixing model would suggest. At the same time, individuals holding a minority preference are more likely to report the experience of disagreement. Thus, just as Warren Miller (1956) demonstrated for American counties in the 1950s, this analysis demonstrates for electoral democracies in the early 1990s. Some political minorities may be resolute and determined, but it would be mistaken to argue that they are consistently able to turn inward in a way that protects them from confronting dominant opinion. To the contrary, even at the level of the nation, aggregate preference distributions penetrate the closely held environments of citizens.

The connection between the political composition of communication networks and the political composition of surrounding populations is only important if the resulting patterns of communication are relevant to the political preferences, opinions, and engagement of the individuals located within the networks. In fact, the influence of political communication networks has been demonstrated relative to a wide range of political behaviors. Patterns of interdependence among and between citizens help to explain levels of tolerance (Gibson 1992; Mutz 2002a); participation (McClurg 2006); public opinion, partisanship, and vote choice (Zuckerman, Dasović, and Fitzgerald 2007; Huckfeldt, Johnson, and Sprague 2004); political ambivalence (Visser and Mirabile 2004; Mutz 2002b; Huckfeldt, Mendez, and Osborn 2004); and more. In short, the composition of networks reflects the composition of surrounding aggregate populations, and network composition has important consequences for the behavior of the individuals located within the networks. This means, in turn, that these networks are directly related to variations in behavioral probabilities across units of aggregation, and hence they provide a key element in explaining the micro–macro divide in democratic politics.

For example, Baker, Ames, and Renno (2006) analyze heterogeneity and disagreement within political communication networks in the 2002 Brazilian election. They argue that, in new democracies where political parties are underdeveloped, politics is more fluid and volatile, and political communication networks thereby become correspondingly more important. Their focus on heterogeneity and disagreement provides important insights into the ways in which volatility is produced and resolved during campaigns. They conclude that deliberation among citizens was crucial to the outcome of the election, and that political communication networks enhanced the civic capacity of both individual citizens and the electorate.³

³ For a complementary analysis regarding Japan, see Ikeda, Liu, Aida, and Wilson (2005).

THE MICRO–MACRO DIVIDE IN POLITICAL CAPACITY

One particular instance of the divergence between individuals and aggregates is worthy of extended consideration in this general context. A distinguished tradition of analysis by Converse (1964), Delli Carpini and Keeter (1996), and others has documented the problematic capacity of individual citizens to function effectively in democratic politics. Low levels of knowledge, an inadequate grasp of ideological concepts, and contradictory belief systems raise fundamental questions regarding the ability of citizens to make democracy work. In pursuing this theme, contributions by Page and Shapiro (1992), Erikson, MacKuen, and Stimson (2002), and others have demonstrated that citizens in the aggregate tend to function more effectively than one might expect based on the cumulative performance of individual citizens. As Sniderman (1993) suggests, this puzzle of capable aggregates and incapable individuals has created a research agenda for students of democratic politics.

Viewed from the perspective of the present argument, this is one more instance of the micro–macro divide in democratic politics. If individuals act independently of one another, then Durkheim was wrong, and the group formed by associated individuals should possess the same characteristics that emerge from the simple summation of the individuals who compose the group. Quite clearly, this is not the case with respect to citizenship capacity, and part of the solution to the puzzle is likely to be based in patterns of interdependence among the individuals who comprise the aggregate.

If this is the case, the early work of the Columbia studies might lead one to expect that politically expert citizens would play an outsized role in the collective deliberations of democratic politics (Katz 1957). And indeed, cross-national analyses based on multiple national comparisons suggest that this is the case. In particular, respondents to social network surveys report higher frequencies of political discussion with individuals whom they perceive to be politically knowledgeable (Huckfeldt, Ikeda, and Pappi 2000). In this way, patterns of political interdependence among citizens may be creating added value in democratic politics; communication that weights the opinions of experts more heavily may indeed create an aggregate opinion that is more politically sophisticated than one would expect based on individual levels of expertise. (For a contrasting view see Zuckerman, Dasović, and Fitzgerald 2007, as well as Zuckerman this volume.)

One potential criticism of such a view is that citizens' perceptions of expertise cannot be trusted. Rather, respondents might engage in motivated reasoning when asked to judge their discussants' levels of competence – they may evaluate levels of competence more generously when the discussant agrees with their viewpoint. Snowball interviews with discussants provide the opportunity to verify the credibility of the respondents' judgments. Contrary to these expectations of biased perceptions based on agreement, analyses of these data show that the most significant factors driving the respondent's judgment of discussant expertise are the discussant's self-reported level of political interest and the

discussant's score on a political knowledge battery, with much smaller effects arising due to the discussant's education level and agreement between the discussant and the main respondent regarding presidential vote choice (Huckfeldt 2001). In short, citizens talk more frequently with other citizens to whom they attribute higher levels of knowledge, and these attributions are driven by reality.⁴

IMPLICATIONS FOR ALTERNATIVE VISIONS OF POLITICS

In his analysis of social theory and social change, Boudon (1986) outlines the role of microsociological models of behavior. According to Boudon's sociological vision, the role of the micro-model is to expedite movement back and forth between macro and micro levels of analysis. The construction of the micro-model is hence of primarily utilitarian concern. There is no single correct micro-model of behavior, no search for a Holy Grail, no loyalty to some other discipline's turf. This position of the opportunistic, eclectic agnostic is one that might indeed be congenial to students of politics, and particularly to students of comparative politics.

Thirty years ago, in the midst of the rationality wars over the presence or absence of instrumental behavior in politics, such a position might have been unimaginable. In the intervening years, however, we have seen a fairly dramatic transformation of positions. A form of soft rational choice became widespread when it occurred to many noneconomists that theories of democratic politics lose meaning absent rational citizens (Carmines and Huckfeldt 1996), and many political economists embraced a new institutionalism that provided a central role to cooperative, adaptive behavior on the part of individuals (Williamson 1975, 2000). Just as important, developments in political and social psychology have produced theories of political and social cognition, thereby providing important insights into the nature of communication, perception, and persuasion that generate powerful leverage on the problems of individual interdependence (Lodge and Taber 2000; Lodge, Steenburgen, and Brau 1995; Lupia and McCubbins 1998).

In such a setting, what should be the basis for evaluating the utility of various individual-level explanations? Returning to the example of political expertise, an adequate micro-model might explain how individuals look beyond their own devices to obtain information, as well as how aggregates appear more informed than the individuals who are their constituent units. In short, the micro-model should provide a vehicle with which to compose the aggregate from the bottom

⁴ Why they talk with experts more frequently is open to more research. The Downsian explanation is that people quite purposefully seek out experts, an explanation that is coincidental with the spirit of Katz (1957). An alternative argument lies in the empirically documented fact that these citizen experts are noisier people – they talk about politics a great deal! And hence, you may talk politics with your associates who are politically expert for the simple reason that they talk more frequently about politics – an explanation that coincides with complex emergent properties and agent-based explanations (Axelrod 1997; Huckfeldt, Johnson, and Sprague 2004) and falls more neatly within a structural rather than a rational choice framework of analysis.

up – out of its individual constituent parts – as well as to decompose the aggregate from the top down – to break it up into its interdependent constituent parts.

Without such a micro-model, *both* aggregate relationships *and* individual relationships will be misspecified and misunderstood. Aggregate capacity will be wrongly inferred to individuals. Individual incapacities will be wrongly inferred to aggregates, and the role of the expert citizen in democratic politics will be undervalued.

In short, a focus on interdependence among citizens in the resolution of these micro–macro problems of political analysis is not the exclusive domain of any particular tradition of analysis – cultural, structural, or rationality. And each of them is in a position to make fundamentally important contributions.

Cultural Theories

The micro–macro focus being proposed fits quite well with the intellectual insight that infuses many cultural theories. Cultural theories are built on the assumption that there is something different about the attitudes and behavior of citizens in, for example, Japan and the United States. And these differences transcend the simple individual-level differences between the two countries: Individual differences based on income, education, and other demographic and nondemographic characteristics do not account for the differences. In this way, cultural theories are built on the premise that the behavioral probabilities are not constant – that they vary across significant units of aggregation, not only across nations but across other units as well.

A problem arises, however, to the extent that the microtheoretical underpinnings do not offer an account, or move toward an account, that explains this cross-cultural variation. So, for example, Key pushed beyond a simple cultural explanation for southern politics in the United States that laid the blame for southern racism on the fact of being southern. That is, he did not view racism as an inherent cultural trait of individual white southerners, but rather as a consequence of whites' hostility based on their anxieties regarding large, politically and socially threatening populations of blacks in particular areas of the South. These anxieties, in turn, generated intense efforts on the part of whites to maintain political control (Key 1949; also see Woodward 1955).

Yamagishi and Yamagishi (1994) offer a similar analytic device in their rich and wide-ranging comparison of Japan and the United States. One lesson to be taken from their analysis is that a widespread tendency toward distrust can be overcome with concentrated investments in social capital (Coleman 1988), defined in terms of extensive networks of social relations. *This generalized sense of distrust is, in fact, the source of investment in social capital rather than the consequence of its absence.* Hence, we return to the complex relationships that connect the organization of social relationships to the distribution of social and political attitudes, orientations, and predispositions within a population.

In a similar vein, Huckfeldt, Ikeda, and Pappi (2005) address the seemingly high levels of political consensus within the communication networks of

Japanese citizens, showing that political agreement within these networks is actually quite comparable to that of citizen networks in Germany and the United States. The real cross-national difference in these networks is in the manner in which *disagreement* is managed. The Japanese report much higher levels of uncertainty regarding their discussants' political preferences, but the level and pattern of agreement – relative to disagreement and uncertainty – are comparable to those of the other nations.

None of this is aimed at suggesting that all cross-group and cross-national differences are readily explainable. In many or even most instances, cross-aggregation differences in individual attitudes and behavior do not simply disappear as the function of any particular explanatory device. The principle, however, persists. A thoroughly comparative analysis seeks to locate the sources of variation in individual behavior in factors that do not simply suggest that southerners or Japanese or Americans are simply different. A resort to this latter device is simply to embrace what Wrong (1961) and Granovetter (1985) refer to as an “oversocialized” view of individual behavior. That is, it locates the source of variation in internalized norms and ideas that become idiosyncratic to the individual psyche.

Structural Theories

Structural theories would appear to offer at least part of the solution to this problem. By focusing attention on the structural conditions that create differences in democratic politics among and between units of aggregation, one would anticipate an analysis that does more than assert the primacy of internalized beliefs and norms. Rather, the underlying premise is that the key to understanding aggregate outcomes lies in the structural and aggregate conditions and circumstances that produce them. Such an analysis is helpful as far as it goes, but a failure to incorporate a theory of individual-level behavior produces a problem similar to that considered earlier.

Returning again to the analyses of Wrong and Granovetter, the problem with any account that lacks a micro motor is a corresponding failure to consider the motives of individuals, and hence an oversocialized view of human behavior. Indeed, the Achilles heel of many structural explanations is the lack of a scientifically compelling microtheoretical explanation for the individual-level factors that produce macro outcomes. Indeed, some analyses of politics in the aggregate start from the premise that we do not need micro information! The problem continues to be, however, that if individual behavior is density dependent and hence variable across aggregation units, neither an individual-level explanation nor an aggregate explanation will be satisfactory. Hence, a structural or cultural theory that does not incorporate an explanation for the behavior of individuals will inevitably fall short, forced to make explicit or implicit assumptions regarding the micro motives that extend beyond available evidence.

The incorporation of network explanations within structural explanations has produced a powerful theoretical response to the problem. As an example,

consider a comparative analysis of street corners during a time of civil disturbances and riots, based on Granovetter's (1978) classic analysis. On any given evening during such a period, riots break out on some street corners and not on others. One way to address the structural causes of such outbreaks is to employ an aggregate analysis that considers the exogenous factors giving rise to the outcomes – local conditions and circumstances, the extent of local grievances, the mean level of discontent, and so on.

In his analysis, Granovetter (1978) employs a threshold model in a mental experiment to consider the conditions under which riots occur. Suppose that each of the street corners involves 100 individuals and that each of the individuals has a propensity to riot that is a function of the other individuals' riot behavior. Some people will riot if no one else is rioting, others when 20 are rioting, and so on. Now imagine that a particular street corner has a gathering of 100 individuals, each of whom is willing to join in the riot after 1 individual has started rioting. In such a situation, no one will riot. Now consider a group in which the willingness to riot is uniformly distributed as a function of aggregate riot behavior, from 0 to 99. In this situation, everyone will riot, even though the aggregate propensity in this group demonstrates much less willingness (where the mean is approximately 50) than the aggregate propensity in the first group (where the mean is 1). In short, whether or not a riot occurs has more to do with the patterns of relationships among political participants and much less to do with the simple aggregated characteristics of the group. (Also see Petersen 2001; Lohmann 1994; and McAdam, Tarrow, and Tilly this volume.)

These sorts of explanations are particularly compelling (also see Schelling 1978) because they incorporate the decisive potential of stochastic factors. Whether or not a riot occurs on a particular street corner may be determined less by the aggregate level of discontent than by whether or not the true revolutionary (with the threshold of 0) happened to show up on a particular evening. In the words of Boudon (1986), "chance is not nothing." Rather, as it is realized through stochastic processes such as these, chance is subject to the structural characteristics of particular settings, and it is apt to be directly related to the micro-motors that tie together individuals and aggregates.

Boudon's (1986) insights into these problems merit careful attention. In his analysis, structures are not necessarily determinate in yielding particular outcomes. Rather, structures give rise to a range of possible outcomes, the likelihoods of which are contingent on structural properties and arrangements. A great virtue of his approach is that it frees structural explanations from the yoke (and criticism) of a heavy-handed determinism, as well as making it possible to incorporate the accompanying uncertainty of individual traits, social interaction, and other sources of stochastic variation within structural explanations (Huckfeldt 1990).

Rational Actor Theories

At least from the vantage point of this chapter, the question has little to do with whether citizens are rational! Along with Weber (1966) and Simon (1985), this

chapter accepts the premise of rationality unless someone or something offers compelling evidence to the contrary. The larger question is the contribution that rational actor theories make toward the resolution of micro–macro problems. And the answer to this question is, of course, that the rationality assumption has made an extraordinary contribution to the identification and resolution of micro–macro issues in political analysis.

Economic approaches to the study of politics have been criticized for introducing an excessively atomistic approach to the study of politics. While this criticism has certainly been appropriate in some instances, it might also be leveled at some, but not all, psychological approaches to the study of politics. Moreover, and as we have argued, there is nothing to be preferred in an entirely aggregate (or structural) approach relative to an entirely individualistic approach. Both are misspecifications that ignore variations in individual behavior as a function of aggregate characteristics – systematic variation in the individual behavioral probabilities across aggregate units of analysis.

Perhaps the most notable contribution of rational actor theories to the resolution of the micro–macro issue is the introduction of the collective action problem to political science audiences by Olson (1965). Olson asks a beguilingly simple question: Why do individuals join political groups? Group theories of politics – theories that played a key role in giving birth to the modern study of political science – made a typically implicit assumption that individuals join groups to advance their own political interests. Olson renders such an assumption obsolete by introducing the free rider problem to the study of politics. Why should I contribute to the success of a political group in a context where (1) my own contribution will not determine the success or failure of the group and (2) I can realize the benefits of the group's political accomplishment regardless of whether I contribute? Olson's analysis made a signal contribution to a theory of political groups, and in so doing he forced a modification in the group theory of politics (Salisbury 1969, personal communication).

The identification of the collective action problem revolutionized the way that political scientists evaluated political groups, but it also revolutionized the way that political scientists think about public goods and political institutions. If political institutions are created to provide public goods in the context of collective action problems, then one can see many institutions as a fundamental political response to micro–macro problems in politics. The introduction of the new institutionalism within economics (Williamson 2000) has encouraged political scientists to expand the definition of political institutions – to identify important, often informal, and typically grassroots responses to collective action problems (Ostrom 1990), and political communication networks might certainly be seen in this context.

In his *Economic Theory of Democracy*, Anthony Downs (1957) addressed the costs and benefits that accrue to the individual from voting, with the clear implication that rational individuals might indeed abstain from voting. A primary cost of voting relates to the information required to make voting a meaningfully beneficial behavior, and Downs – citing the work of Berelson,

Lazarsfeld, and McPhee (1954) – considers the possibility that it might be entirely rational to reduce information costs by relying on information taken from informants whom an individual judges to be both politically expert and politically trustworthy.

Seen this way, voting becomes another form of the familiar collective action problem, and political communication networks provide a vehicle to resolve the problem. By dramatically reducing information costs, these networks provide a device whereby individuals are encouraged to participate. Moreover, as Ostrom, Walker, and Gardner (1992) show, nonparticipants in the resolution of collective action problems can be punished by their associates for their failure to cooperate. And individuals who are located in politically activated discussion networks may experience a subtle (or not so subtle) form of coercion that drives them to the polling place.

Regardless of whether networks are seen as institutions or voting as a collective action problem, it becomes clear that participation within a network of political communication is coincidental with rational actor theories of politics. Participation within such a network need not necessarily be directed toward an instrumental end; participation may be an activity that is valued in itself (Fiorina 1990). Some people enjoy baseball, others enjoy sitcoms, and still others enjoy talking politics. The important point for present purposes is that these networks enhance political communication and engagement, allowing many citizens to obtain information on the cheap.

The information conveyed through these networks is never neutral, inevitably reflecting the biases of the individuals located within the networks. Hence, wholly rational behavior leads people to be informationally interdependent with others, and hence to hold perceptions and preferences that are variable across networks anchored in space and time. It comes as no surprise, once again, that the likelihood of engaging in various political behaviors varies across space and time, with the implication that a multilevel analysis of politics is dictated not only by cultural and structural theories of politics, but by rational actor theories as well.

A COMPARATIVE VISION OF DEMOCRATIC POLITICS AND THE VOTE: A REPRISÉ

Where does this analysis leave us with respect to the vote – the behavior often seen as the fundamental act of citizenship? The view of democratic politics put forward here is inspired by a view of the individual vote as the culmination of a social process (Przeworski and Sprague 1986). From this perspective, explaining the vote or any other preference as a reconstruction of individual characteristics or individually held interests is a futile undertaking. Preferences must ultimately be informed; they do not arise as an automatic, predetermined translation of individual characteristics and interests. In this context, information is crucial, and the stream of incoming information is particular to an individual's location in time and space.

One might, of course, reconstruct the vote with a sufficiently rich record of individual cognitions, conceptions, preconceptions, impressions, and goals. This is an infinitely worthwhile undertaking, addressing the cognitive processes that translate mental images in long-term memory into a summary judgment (Berent and Krosnick 1995). The research program advocated here would address the summary judgment, as well as the cognitive process leading to it, within the boundaries of the underlying processes that gave rise to both (see Lodge, Steenbergen, and Brau 1995; Lau and Redlawsk 2006; Pattie and Johnston 1999). That is an admittedly ambitious undertaking, and it gives rise to several implications.

First, it is at least conceivable that we misunderstand the importance of the vote as an isolated event. By focusing on the vote to the exclusion of the process that produces it, we run the risk of undervaluing democratic politics – a collective process rooted in highly complex forms of social interaction, cognition, and the transmission of political information.

Second, it may be time to reconsider political communication and political information as central ingredients of the political process. The study of political participation has traditionally been defined in terms of activities aimed at influencing the selection of officeholders and the policies they adopt. Hence, not only has the vote been separated from the social process upon which it is predicated, but this process – defined in terms of information and communication – is too often ignored as a central ingredient of the way in which citizens participate in the political process.

None of this is meant to minimize the importance of the vote or to suggest that the vote decision should no longer be studied as a central outcome in democratic politics (see Huckfeldt and Sprague 1995). Rather, the view being constructed here embeds the vote in a complex social and political process. While for many purposes it may be useful to view the vote in abstraction from the processes and events upon which it is predicated, the parameters of the process are particularly important to the comparative study of democratic citizenship. That is, the primary sources of variation in the exercise of citizenship are related to variations in information and communication anchored in time and space, and hence they become crucial to a comparative understanding of democratic politics.

CONCLUSION: THE IMPORTANCE OF SPECIFIC PATTERNS OF SOCIAL RELATIONS

As long as individual behavior does not vary as a systematic function of aggregate characteristics and attributes, one need not be concerned with the argument laid out in this chapter, and either a micro or a macro focus will yield consistent and unbiased results. One might add that, in such a situation, the study of comparative politics takes on much less urgency. If individuals are not interdependent and if behavioral probabilities are spatially invariant, then differences in democratic politics across nations can be wholly explained as a consequence of the idiosyncratic social and political compositions of the particular populations.

This rather provocative conclusion is, of course, wholly irrelevant. Comparative politics is a vital and productive area of study for the simple reason that politics is *not* reducible to the sum of its parts. The irreducibility of politics means, in turn, that the study of politics is not readily subsumed by either a micro or a macro focus. Rather, the focus of political analysis, within and beyond the disciplinary boundaries of comparative politics, can productively entertain multilevel analyses of political processes. And these analyses will often provide insights that reflect the richness and subtlety of the underlying phenomena. Indeed, the aggregate contingencies that are imposed on individual behavior make a thoroughly comparative analysis particularly compelling. In this sense, the comparative analytic stance becomes one of the important contributions made by the study of comparative politics to the remainder of the discipline.

Ecological fallacies are not simply statistical problems, although they certainly benefit from more sophisticated statistical models. Rather, ecological fallacies, as well as their companion individualistic fallacies, point to the importance of a thoroughly comparative understanding of citizenship in democratic politics.

The primary challenge to a thoroughly comparative analysis is a specification of the theories that connect individuals and aggregates. These theories provide political analysts with the means to move back and forth between micro and macro analyses of politics, and in particular to view political aggregates as the emergent consequences of the patterns of connections among individuals (Huckfeldt, Johnson, and Sprague 2004). As Granovetter (1985) has argued so persuasively, in order to provide a compelling account, such theories will benefit by drawing on specific patterns of ongoing social relations. In this way, political analysts move beyond explanations rooted in the psyches, utilities, and internalized norms of individuals to a comparative explanation of particular actors who behave in ways that are contingent on communication, information, and the behavior of others in their surroundings.

Nested Citizens

Macropolitics and Microbehavior in Comparative Politics

Christopher J. Anderson

The focus in the behavioral study of politics is on individuals. As a subfield of political science, it examines actions (e.g., voting and protest) as well as cognitions (perceptions, attitudes, and beliefs); and as a subfield of comparative politics, it examines them in one, several, or many different countries. It encompasses the study of both cognition and action, in large part because we have long believed that attitudes and beliefs explain action, but also because we are convinced that, in addition to formal institutions or processes, cognitive elements of politics such as legitimacy, values, or grievances are important indicators of the quality and nature of democratic and political life.¹

This chapter reviews the intellectual foundations of the behavioral study of politics, and it does so with an eye to its evolution as a subfield of comparative politics. It also discusses the field's affinities with different theoretical traditions in political science and comparative politics. I discuss both the past and present of how we study comparative mass politics and describe how it has been transformed because of changes in technology and intellectual trends and in reaction to real-world events. I argue that, as a result of these changes, the study of mass politics has become more central to the study of comparative politics through its ecumenical relations with different theoretical traditions in the social sciences and comparative politics.

MIXED ANCESTRY: ORIGINS OF RESEARCH ON COMPARATIVE MASS POLITICS

Two intellectual projects underlie the comparative study of behavioral politics. The older one of these is sociological in orientation and has sought to

¹ As Sam Barnes pointed out in the first edition of this volume, "[t]here are, of course, many ways to study mass publics, including the analysis of social movements. . . . Still others include the use of electoral, demographic, economic, and related aggregate data; events data; content analysis; extended open-ended interviews; and participant observation and other anthropological approaches" (Barnes 1997: 115). The principal technology used in the field of mass politics is the mass survey, based on a random sample of individuals drawn from the population of interest – typically all adults or eligible voters.

I wish to thank Sid Tarrow for asking me whether I was a rationalist or a structuralist. Many thanks also to Alan Zuckerman for his generous and thoughtful comments on an earlier draft.

understand the connection among social structures, voters, and political parties (Barnes 1997). In fact, the field's long-standing concern with social structures and their effects on behavior dates back to the first half of the twentieth century (e.g., Tingsten 1937; see also Elff 2007) and originally focused on the connection between social environments and structures, on one hand, and political action and cognition, on the other (e.g., Lazarsfeld, Berelson, and Gaudet 1948; Scheuch 1969; Przeworski and Soares 1971; Sani 1976; see also Huckfeldt this volume).²

While political sociology was the birthplace of comparative mass politics, studies growing out of this tradition were not usually aimed at replacing country names with variable names. Instead, they sought to connect social processes and structures within countries to behaviors of interest, such as voting, participation, and the like (for an exception, see Verba, Nie, and Kim 1978).³ The study of how political cleavages, and in particular the class cleavage, structure the connections between voters and political parties is closely related to this intellectual tradition, and both are closely connected with the study of electoral volatility and party system change. Perhaps the most famous exemplars of these research traditions are Lipset and Rokkan's (1967) study of cleavage structures and voter alignments in Europe and Bartolini and Mair's (1990) investigation of aggregate electoral change over relatively long periods of historical time.

Although sociology provided the intellectual foundation for the study of behavioral and electoral politics, over time students of mass politics came to pay less attention to sociostructural factors and instead shifted their attention to a second approach rooted primarily in psychological concepts as principal explanations of citizen behavior (Dalton 1984; Dogan 1995; Franklin 1985; but see, for example, Verba, Nie, and Kim 1978). In part, this psychological turn of the field had to do with the apparent decline of the structuring power of political cleavages in advanced industrialized societies during the 1960s and 1970s. Class, religion, and region appeared to become less potent predictors of vote choice and political preference, and students of mass politics started to look elsewhere for empirical and theoretical traction (Dalton, Flanagan, and Beck 1984; but see Elff 2007 for a more recent argument and evidence).

The reasons for the growing neglect of structural influences on citizen behavior at the time go beyond the apparent weakness of the sociological approach, however, and almost certainly have had to do with the nature of the behavioral revolution in survey research, its export of a particular model from the University of Michigan to other countries and to comparative politics. By design, the Michigan approach focused attention on the actions of citizens as autonomous individuals whose group memberships and social contexts were

² An important question in this research has been the problem of the so-called ecological fallacy and the challenges associated with drawing inferences about individuals' behavior from aggregate data (Robinson 1950; King 1997).

³ These earlier analyses often focused on level-of-analysis questions and very prominently on the so-called ecological fallacy – that is, the difficulty of drawing inferences about individual behavior based on aggregate data (Eulau 1977).

conceptualized as being experienced individually rather than structurally. To be sure, this approach was a first cousin of the sociological one: It originally drew on sociology as an explanation for the origins of voters' beliefs (in the form of the famous funnel of causality). And while social structure thus continued to be a conceptual part of the overall conception of voter behavior by influencing it indirectly, this approach essentially became synonymous with psychological concepts (such as partisanship) as the most proximate and principal explanatory factors. As a result, researchers came to pay less attention to sociostructural contexts such as neighborhoods, communities, or countries that individuals inhabit and more attention to the cognitive processes underlying the political choices people make.

In the decades that followed, this individualized and cognitive approach to understanding citizen politics became the dominant approach to political behavior and was exported to various areas of the globe, perhaps most successfully to Western Europe (Kittilson 2007). As importantly, the phenomenal success of the Michigan approach and its focus on mass surveys helped to establish both an intellectual and an organizational infrastructure capable of supporting the technology of survey research that was well positioned to advocate the theoretical foundations of the Michigan paradigm.

Thus, the psychological approach was a natural descendant of the sociological one. Although they privileged different explanatory mechanisms, both approaches tended to focus on explaining variation within countries. And the sociological tradition was always lurking in the background, especially among European researchers or researchers interested in explaining the apparent decline in the power of traditional cleavages; but during much of the post–World War II period, the psychological approach and the technology of sample surveys held sway.⁴

This particular intellectual foundation of behavioral politics meant that, despite the international diffusion of survey research, projects aimed at comparing behavior across countries with the help of explicitly cross-national surveys with comparable measures collected at similar points in time were quite rare. Instead, a proliferation of single-country studies were theoretically and empirically focused on psychological concepts like partisanship, alienation, or efficacy, and were intended to probe a concept's usefulness within a particular country context rather than to model its variability across countries. (Notable exceptions included Almond and Verba's [1963] *Civic Culture* study, Barnes et al.'s [1979] *Political Action* study, and Inglehart's [1977] work on value change, for example.) As well, a series of national election studies in individual countries were designed to apply the Michigan model of voter behavior to specific elections and countries. Even when researchers made use of cross-nationally comparable surveys, the underlying

⁴ This classification is admittedly somewhat arbitrary, especially in light of the fact that the study of social psychology – a close relative of behavioral politics – is both sociological and psychological in nature and once upon a time was also formally unified within sociology or psychology departments.

factors driving behavioral outcomes were frequently assumed to be common to a similarly situated set of countries. Thus, these studies were considered “comparative” mostly because they were conducted outside of the United States or in a set of (relatively similar) countries, not because there were strong theoretical reasons to believe that the individual-level relationships differed across countries. Perhaps the most famous examples of such debates centered on questions of whether partisanship can travel to countries other than the United States (Blais, Gidengil, Nadeau, and Nevitte 2001) or whether class was universally in decline as a determinant of voter behavior (Evans 1999; Elff 2007).

Given the focus on individual-level concepts whose investigation did not require cross-national variation, this was not surprising. But two of the major downsides of this approach were a tendency of behavioral researchers to focus on variation within populations rather than across countries, and of conversations to focus on quite technical matters of survey methodology and concept measurement. In hindsight, this made it all too easy for scholars working in other areas of comparative politics to overlook insights from mass politics research for their own research agendas, and for behavioral researchers to use their technology and insight to enter major debates in comparative politics that went beyond the study of electoral politics in stable and rich democracies.

This situation started to change as comparative behavior research began to move from a dominant focus on micro-level processes to macro-level manifestations of behavior – that is, on comparing the aggregate attitudes and reported behaviors of citizens across countries with the goal of unearthing their macro-level, cross-national causes and consequences (Inglehart 1983). In part, this development was made possible by the emergence of several collaborative cross-national survey projects in the 1970s and 1980s designed to allow researchers to compare the attitudes and behaviors of citizens in many countries around the world (see also Dalton 2000) (such as the Eurobarometer Surveys, the World Values Surveys coordinated by Ronald Inglehart, or the International Social Survey Project; see Kittilson 2007).

This movement toward more explicit coordination of survey data collection across countries and the ability to systematically compare what citizens thought and did across countries fortuitously coincided with other trends in the real world of politics and inside the ivory tower: the rapid expansion of electoral democracies around the world in the 1980s and 1990s, as well as a renewed focus on both institutional questions in Comparative Politics and contextual theories in the study of political behavior (cf. Hall and Taylor 1996; Zuckerman 2005b).⁵ On the intellectual front, the renewed interest in institutions among students of comparative politics left its mark on behavioral research by shifting attention away from social structure as the major contextual influence on mass behavior to institutions and policy outcomes. At the same time, a renewed interest among students of political behavior in the link between social context and electoral

⁵ Another helpful development entailed significant advances in desktop computing technologies and statistical techniques appropriate for conducting cross-national research (Anderson 2007a).

behavior provided an impetus to view citizens as nested in nonneutral contexts, and it furnished a theoretical model for understanding contextual influences more specifically (cf. Huckfeldt this volume).⁶ As a result, recent years have seen a clear move toward more systematic cross-national research, increasing attention to the impact of formal institutions on behavior, and the systematic testing of contextual theories of behavior in a broader universe of countries. Taken together, these shifts have opened up opportunities to integrate behavioral politics with institutional politics across a wider range of theoretical concerns in comparative politics.

THE PIECES OF THE PUZZLE: MULTILEVEL MODELS IN COMPARATIVE RESEARCH ON MASS POLITICS

A first consequence of the increased availability of cross-nationally comparable surveys and electoral data was the publication of a number of studies that focused at the macro level across countries, typically by examining the impact of countries' institutions, structural conditions, or political and economic outcomes on aggregated measures of behavior or attitudes. In this vein, perhaps the most extensively researched country-level institutional feature has been a country's electoral system and how it shapes voters' choices to turn out to vote or which party or candidate to support.

For instance, a small industry of researchers has investigated the cross-national correlates of turnout in modern democracies, including several features of the electoral rules, such as registration requirements, the frequency of elections, compulsory voting, the timing of balloting, the proportionality of the electoral system, or district magnitude (see Gray and Caul 2000; Franklin 2004; Powell 1986; Jackman 1987). As well, a plethora of studies have found that different electoral rules produce systematically different election outcomes, measured by the effective number of parties, electoral volatility, or the success of parties and candidates with particular characteristics (see, e.g., Duverger 1954; Rae 1967; Pedersen 1983; Riker 1982b; Bartolini and Mair 1990; Lijphart 1990; Cox 1997).

The models of behavior underlying these streams of research usually imply that the effects of institutions (or other macro-level characteristics) are direct – that is, they presume that citizens' decisions are directly affected by the incentives that the rules or context provide. Thus, higher levels of turnout in countries with less frequent elections, for example, are accounted for by the fact that the structural features that individuals are exposed to – election frequency – directly affect the incentives people have (in this example, the costs of going to the polls). At first glance, many comparative studies of political behavior resemble such a simple “direct effects” model, at least empirically. The studies of aggregate voter turnout mentioned earlier or the study of electoral volatility are examples of such models. Yet, upon closer inspection, many turn out to be based on theoretical

⁶ For an excellent introduction to the politics of context, see Huckfeldt (1986).

models of indirect or conditional effects, even if the empirical analyses investigate the impact of institutional features on behavior as if it were direct.

Theoretically speaking, indirect effects imply that structures affect some intervening variable, which, in turn, is the actual proximate cause of the dependent variable. We could hypothesize, for example, that the degree or kind of ethnic heterogeneity in a country affects citizens' identification with their ethnic group, which, in turn, affects whether they engage in peaceful or violent political action. Or particular cleavage structures lead voters to develop strong class identities, which, in turn, shape their attachments to political parties. In these examples, theoretically and empirically speaking, structure has consequences – here, how heterogeneity shapes individuals' ethnic identification or how social structure affects class consciousness – and these consequences, in turn, have secondary effects on behavior.

In addition to such indirect effects, the impact of structures can and should often be viewed as conditional. Conditional effects models imply that the effect of some structural feature on voter behavior is strengthened or weakened, depending on the presence of a third variable, such as voters' priorities or preferences. For example, while electoral rules may increase the costs of voting across all voters, these costs have effects of differing magnitude on individuals' decisions to turn out to vote because individuals with different levels of resources are differentially able to bear these costs. Alternatively, structure can be the intervening variable that helps determine the relative strength that an individual-level independent variable may have on behavior. To use the electoral system example once more, individuals' resources should affect the odds of turning out, but this connection may be particularly strong in countries with electoral rules that make turning out more costly.

It is important to note that conditional or indirect models, on the one hand, and simpler direct effects models, on the other, are not necessarily at odds with each other. But the former go beyond the simpler earlier models in that they represent a more finely grained theoretical and empirical specification of the underlying causal mechanisms at work. Thus, direct effects that institutions or structures are shown to have on a behavioral outcome – say, turnout – may simply be an average for the population as a whole that hides significant heterogeneity in people's behavior. Similarly, the connection between an individual-level variable and individual action may be differentially strong across countries for reasons that could be specified theoretically and modeled empirically.

Recent years have seen a surge of research using so-called multilevel models,⁷ which seek to model these more complex and heterogeneous relations by combining different levels of analysis: the macro level across countries and the

⁷ These models are also sometimes referred to as “hierarchical models.” I prefer the term “multilevel” or “contextual” to “hierarchical” in part because the former indicates the focus on different levels of analysis (individual and country, for example) and because the latter has specific connotations in the context of recursive regression analyses, and because it requires that some variables are exogenous and others endogenous.

micro level of citizens' attitudes and actions (Anderson 2007a). This means that such models of citizen behavior go beyond simple cross-national studies that correlate countries' macrocharacteristics and aggregated responses to survey questions of interest or aggregate election statistics. Multilevel models are particularly well equipped to integrate what we know about citizens' political behavior with what we know about macro-level differences across countries because they connect the individual-level experiences people have as participants in the political process and how they interact with the constraints any particular political system or situation provides in the form of structural, institutional, or cultural contextual parameters (Kedar and Shively 2005). Such models constitute a major step forward in the comparative study of behavioral politics because they allow us to specify this heterogeneity both theoretically and empirically.

SOME EXAMPLES: REPRESENTATIONAL STRUCTURES AND VOTER BEHAVIOR

One area in which the role of cross-national macro-level variables has been examined extensively is in research on economic voting. In recent years, students of electoral politics have addressed the puzzling finding that the economy does not exert consistently strong, and sometimes even counterintuitive, effects on election outcomes across countries or at different points in time (Paldam 1991).⁸ To explain this empirical inconsistency, a number of recent studies have focused on how the nature of a country's representative structures interacts with voters' willingness to punish governments for bad economic performance to produce different election outcomes. Most of this literature argues that the impact of a bad economy hinges on the ability of voters to assign responsibility to governments for economic performance. This ability has come to be thought of as being affected by structural features of politics, which act as institutional barriers that make it difficult for voters to obtain the necessary information about the representative's activities and the connection between their activities and economic outcomes.

At its core, this understanding of the connections among economic outcomes, political institutions, and voter behavior is based on a theoretical model of conditional relations. For example, institutions are complex and allow representatives to avoid blame – an example is the frequent practice of coalition government in the continental European countries – and this makes it difficult for voters to figure out who is to blame for a bad economy. In this case, institutions act as a moderator by facilitating or hampering the relationship between citizens' motivation to reward or punish governments and their vote choice. As a result, the impact of the economy – in the form of individual voters' evaluations of the economy, for example – on voting behavior for or against the government will vary in strength and perhaps even in direction, depending on the institutional context. And we can also imagine that macroeconomic

⁸ This puzzle was foreshadowed in Eulau and Lewis-Beck (1985) and Lewis-Beck (1988).

performance – a macro-level variable – affects voters' choices to different degrees, depending on their sophistication or their ability to discern policy responsibility. In this latter example, individual heterogeneity in sophistication moderates the impact of a macro-level variable on individual vote choice.

Demonstrating and disentangling these relations has been the subject of a growing body of research in comparative electoral politics (see Anderson 2007b for a review). In revising the traditional model of economic voting, attempts to incorporate representational structures more explicitly were pushed along by the publication of a paper by Powell and Whitten (1993), who classified political systems into those where responsibility is clear and those where it is not. This classification is based on formal institutional and structural characteristics of countries such as one-party versus multiparty rule, whether there is bicameral opposition, decision-making powers for opposition parties in parliament, or party cohesion. Their analysis found that economic effects were stronger in those countries that had clearer levels of responsibility.⁹

A complementary perspective on how differences in political context may moderate the relationship between economy and government support views clarity of responsibility as variable over time within (and across) countries because of election outcomes that change the balance of power and elite bargaining, both of which periodically reshape the political context in which voters seek to affix credit and blame (Anderson 1995a; Nadeau, Niemi, and Yoshinaka 2002). Thus, even when formal institutions do not change or vary, the extent to which voters are able to assign responsibility to political actors changes from one election to the next and in between because of the political dynamics created by electoral systems, party systems, the process of government formation, and the like (Anderson 1995a, 2000).

Finally, this new contextual branch of economic voting research has documented that the ability of voters to retain or throw incumbents out of office is also contingent on the presence of credible alternatives. Yet, unsurprisingly, perhaps, the extent to which voters have or perceive such choices varies considerably (Anderson 1995b; Bengtsson 2004). For example, fragmented party systems, volatile party systems, or party systems dominated by one dominant party should make it more difficult for voters to identify a clear alternative to the incumbent government (Anderson 2000; Paldam 1991). In such countries, there tends to be greater uncertainty about the likely shape of an alternative future government that will form after the election has been held, and this results in a

⁹ The story I tell here can easily be made more complex. For example, an additional factor has been added by Pacek and Radcliff (1995), who find that more extensive welfare states cushion the impact of a bad economy on the vote. In addition, scholars recently have begun to examine the impact of the international economic environment on economic voting (cf. Hellwig 2001), and Duch and Stevenson (2005) have introduced the notion of competence signals as a factor that makes voters more or less likely to punish and reward governments. Finally, Samuels (2003) recently has pushed the Powell–Whitten classification to include the distinction between presidentialism v. parliamentarism (as well as the existence of concurrent or nonconcurrent presidential and legislative elections).

diminished likelihood that voters will turn out the incumbent government even when economic conditions are bad (Anderson 2000).

While the different approaches to understanding contingent effects from an institutional or contextual perspective differ – some focus on formal features of a political system while others focus on the dynamics of the macropolitical context of the day, some focus on incumbents while others focus on the alternatives voters have, some are based on a combination of macro-level and individual-level data and others use exclusively cross-national aggregate data – they are based on similar theoretical models that combine cross-national institutional differences with individual-level differences to produce individual-level outcomes. They all view voters as willing to reward and punish, but as being thwarted by contextual conditions inherent in processes and institutions of representation.

The basic insight of this work – namely, that a country's democratic design affects voters' behavior – has been pursued in other domains of electoral behavior as well. For example, Huber, Kernell, and Leoni (2005) examine the relationship between institutional features and partisan attachment across 25 new and established democracies around the globe. They find that institutions that encourage retrospective clarity of responsibility foster the formation of party attachments. Moreover, institutional context has differential effects: Features of political systems that make it more difficult to form party attachments have their biggest impact on individuals with the fewest cognitive resources.

While she follows a different empirical strategy, a similar theoretical model underlies Kedar's (2005) work on how institutional context conditions the relationship between voter goals and voter behavior. Comparing two highly majoritarian democracies and two very consensual ones, Kedar finds that voters incorporate the way institutions convert votes on policy into their choices. Since policy is often the result of institutionalized multiparty bargaining, which waters down the translation of policy preferences into policy outcomes, voters compensate for this by supporting parties whose positions differ from (and are often more extreme than) their own.¹⁰

To give yet another example of the kinds of insights multilevel approaches can generate, electoral institutions and the outcomes they produce can lead to reinterpretations of what we know about the effect of macro-level institutions and structures on voter behavior. For example, Brockington (2004) and Jusko and Shively (2005) reexamine the effect of party systems and coalition government on voter turnout to see if the impact of party systems differs for different kinds of voters. Consistent with much research on the effects of proportional representation on turnout reviewed earlier, they find that, for high-information

¹⁰ In related studies, Klingemann and Wessels (2000) and Gschwend (2007) examine the likelihood that individuals will vote sincerely or strategically. They find that voters' tendency to vote sincerely or strategically depends on institutional characteristics, such as district magnitude, proportionality, allocation rules, and the party system (supply). In particular, they argue that sincere voting is more likely among individuals living in countries with electoral systems characterized by large district magnitude and a high degree of proportionality.

voters, participation in elections rises as the number of parties in the system increases. Thus, among these voters, more choice improves participation rates. However, for citizens with more limited political information, increases in the number of parties in party systems depresses voting turnout. This leads to the ironic conclusion that proportional representation, which is normatively intended to lead to a more fair representation system, increases the information gap by complicating political choices and thus disenfranchises the less informed relative to the better informed.

These examples demonstrate that the systematic incorporation of well-known institutional features into models of political behavior can produce novel insights and help resolve well-known theoretical puzzles or empirical debates – Why does a bad economy sometimes lead voters to throw governments out of office but sometimes does not? Why is turnout higher in some proportional representation systems than others? Why do scholars find support for both proximity and directional models of voting? – and they add a richness that models based on direct and unconditional effects do not possess.

THE TIES THAT COULD BIND: NESTED CITIZENS AND STRUCTURALISTS, RATIONALISTS, AND CULTURALISTS

The building blocks of multilevel models that integrate context and individual behavior are familiar to structuralists, rationalists, and culturalists, and the implicit assumptions that underlie them are put in relief when considered in light of other theoretical approaches. These assumptions suggest that the behavioral study of comparative politics in fact has much in common with structural, rational, and cultural approaches and holds the potential for even further cross-pollination in the future.

When thinking about the ways in which multilevel models view the interaction of structures and individual characteristics to produce behavior, it is useful to start by construing “context” or “structure” broadly. We can think of these as encompassing the immediate social environment as well as macro-level structures at the level of countries or even beyond. And following the definition of an institution as any socially imposed constraint upon human behavior or the “rules of the game” for human interaction (North 1981), they do not have to be formal or written rules – or so-called parchment institutions (Carey 2000) – but can also include informal constraints, such as widely accepted norms of behavior that have long been the focus of cultural theories of politics.

Returning to the origins of behavioral politics for a moment, it is clear that the sociological approach for many years held a dominant position in part because it provides a unifying framework for studying voters, parties, and government behavior. It takes context seriously and seeks to generalize, but it also focuses – at least conceptually – on individuals as the ultimate decision makers. Viewed in this way, the affinity between structuralists and multilevel behavioral researchers is thus long-standing and obvious: They share an interest in generalizability, they combine an interest in social and political institutions with an

interest in behavioral outcomes, and they examine the connections between voters, parties, and governments in institutional contexts.

And like rationalists, behavioral researchers, who, for the most part, use sample surveys to collect their data, are committed to methodological individualism and assume that people act to maximize their advantage. At a minimum, they presume that actors employ reason to achieve their goals and that people's actions can be explained by the motivation to pursue goals. For example, the research on economic and strategic voting reviewed earlier assumes, sometimes explicitly, that voters seek to reward and punish governments in ways that are consistent with their own (or others') well-being, or that they seek to maximize the odds that their preferred policy packages will be implemented. In addition, the multilevel approach instinctively looks for institutional (and other) constraints on goal-directed behavior. Rationalists would have little to quibble about with this model of human behavior (see also Levi this volume).

Finally, while the kinship with culturalists is less well developed and perhaps less obvious, there is no doubt that behavioral researchers have long been interested in political culture, values, identity, and, more fundamentally, the subjective dimension of politics. One way to connect culture and multilevel models is to think of culture as a kind of structural variable that constrains choices by circumscribing what individuals can imagine. As Barnes noted in the first edition of this volume, "Culture can constrain behavior as much as institutions can. It rewards some behaviors and sanctions others. Like institutions, it conditions behavior, it conditions choice" (Barnes 1997: 119). Such an understanding is not all that far removed from the idea of informal institutions (Helmke and Levitsky 2006). If viewed in a structural way, then, culture is easily integrated into multilevel models, at least conceptually. And to the extent that values are the individual-level manifestations of culture, then they, too, are easily assimilated into multilevel models of politics in that they can be seen as moderating the impact of structures or self-interest.

While potential and actual ties between comparative mass politics and the different theoretical traditions in comparative politics are thus real or at least apparent, it is easy to take the metaphor of analytical kinships too far, however. There clearly are important differences between pure versions of culturalists, rationalists, and structuralists, on the one hand, and the way mainstream behavioral political science is practiced, on the other. For example, the kind of rationality that presumably underlies political behavior is clearly a soft form of rationality as goal-directed action, broadly defined. And self-interest is not completely exogenous to behavioral models; traditionally, behavioral researchers have conceptualized self-interest as arising from structures (think class and other cleavages) or with regard to material self-interest, which itself is a product of social, economic, and political structures, historical events, and thus ultimately human choices.

What is more, there is plenty of evidence that voters frequently do not act to maximize their own narrow and short-term self-interest, and instead behave in ways that emphasize collective well-being and other-focused concerns (see also

Zuckerman this volume). Ironically, this is even the case in studies of economic voting, which have found that people's evaluations of the nation's well-being are more potent predictors of vote choice than evaluations of personal material well-being (Anderson 2007b). Similarly, in studies of spatial models of voting, the puzzle to be solved often is why, in apparent violation of narrowly defined self-interest, voters do not consistently choose parties or candidates that are closer to their ideal position (cf. Kedar 2005). And finally, comparative behavioral research goes beyond the narrowly rational choice sort focused on behavior as revealed preferences, which has little use for attitudes and perceptions or the origin of tastes and the formation of preferences.¹¹

Returning, for a moment, to research on economic voting to illustrate the difficulty of employing a strictly rational perspective, there are reasons to doubt that a simple version of rationality is sufficient to account for variations in economic voting that appear in the data. Aside from the fact that voters' behavior is not always obviously self-interested, recent behavioral work in political science rooted in behavioral decision making (at its core, a combination of psychology and economics) (Lupia, McCubbins, and Popkin 2000) would have trouble swallowing some of the assumptions of what voters know when they engage in economic and strategic voting. For example, a basic assumption underlying the studies reviewed earlier is that voters are generally informed about the state of the economy, that this information is unbiased (at least in the aggregate), that they have a sense of the processes by which government action relates to micro- and macroeconomic outcomes, and that they judge electorally accountable decision makers mostly with an eye toward the performance of the (objective) economy. Given existing evidence about voter sophistication and political interest, these are truly heroic assumptions (for a more extensive discussion, see Anderson 2007b).

To begin with, accurate "objective" information about the economy is frequently difficult to come by for the average citizen, and the interpretation of "objective" facts about the state of the economy is frequently contested (Keech 1995). Most people learn about the national economy indirectly from mass media, which tend to overreport negative economic conditions (Goidel and Langley 1995). These negative reports heavily condition voters' economic perceptions (Hetherington 1996), and economic evaluations consequently derive to a greater extent from the way in which the media present economic developments than they do from objective changes in the real economy (Sanders and Gavin 2004; see also Nadeau et al. 1999).

It should not be surprising that all this makes it difficult for voters to form an accurate perception of actual economic conditions in the country. In addition, aside from biases and limits in accurate information, citizens' cognitive limits reduce the extent to which objective information is likely to be coded accurately (Krause 1997; Krause and Granato 1998; however, also see Sanders 2000).

¹¹ To be fair, an increasing number of economists are becoming interested in going beyond revealed preferences, but they still constitute a minority of scholars.

Moreover, citizens do not learn about different aspects of the macroeconomy in the same way or at the same pace (Nannestad, Paldam, and Rosholm 2003; Weatherford 1983). For example, differently situated voters are exposed to systematically different economic experiences that help determine how they perceive the state of the economy (Duch, Palmer, and Anderson 2000; Holbrook and Garand 1996; Weatherford 1983; Welch and Hibbing 1992). As well, lag times in citizens' learning of economic trends differs significantly for inflation and unemployment, for example (Conover and Feldman 1986), and significant proportions of voters are predominantly static and myopic in their evaluations of economic conditions (Paldam and Nannestad 2000). What is more, citizens' own political biases and values work against a close relationship between perceptions of economic conditions and economic evaluations at the individual level. For example, partisans form evaluations of the state of the economy to be consistent with their previously held beliefs (Anderson, Mendes, and Tverdova 2004; Evans and Andersen 2006; Wlezien, Franklin, and Twiggs 1997). Moreover, in contrast to the image of a coolly rational voter, affective reactions to personal and national economic conditions have been found to shape economic evaluations (Conover and Feldman 1986).

All these facets of how voters think about the economy have important implications for how we think about rational voters. At a minimum, this research undermines the assumption that significant portions of the electorate judge a government's record based on what occurs in actuality. And this is at odds with narrow rational models of voter behavior, and it also is plausibly consistent with competing theoretical approaches. Thus, even in the area of economic voting, the affinity between students of mass politics and rationalists, while apparent, is not entirely tight-knit.

The same can be said about the connection between structuralists and multilevel behavioralists. Both view social and economic structures and institutions as important elements in shaping behavior, but behavioralists emphasize the probabilistic nature of this connection, while pure structuralists make it difficult for individuals to escape the constraints that structure dictates. Thus, behavioralists interested in individual-level heterogeneity are slightly less mechanistic or deterministic than simple versions of structure might imply. Students of comparative mass politics have only recently rediscovered the long-standing connection between structural and behavioral approaches in a way that eschews the conceptual determinism of some earlier approaches. Here, Zuckerman, Dascović, and Fitzgerald's (2007) work on families and partisanship or the comparative studies of social networks and electoral behavior (see also Huckfeldt this volume) are representative of the promise inherent in this research agenda.

Behavioral scholars share with structuralists and rationalists the tendency to stress generalizability. Although this stands in contrast to culturalists' instinct to shy away from generalizing beyond the single case, it does not necessarily imply a kind of natural antipathy between behavioral and cultural researchers. In fact, there is a good deal of common ground on which to build. The connection between students of behavioral politics and those engaged in cultural studies is

that they privilege the subjective dimension of politics through the study of perceptions, attitudes, and shared understandings. At the individual level, culture is a subjective phenomenon and is concerned with interpretation and understanding of self and others. The subject of identity, for example, comes to mind as something that both behavioral and cultural researchers can and should reasonably claim.

To think about the connection between culture and voter behavior in the context of a concrete example, recall the research on economic and strategic voting reviewed earlier, which is based on arguments that draw on the themes of rationality and (political) structure. This literature assumes that voting (and related activities) are instrumental acts of rational, self-interested citizens. But suppose, instead, as a culturalist might argue, that voting (and other acts of participation) are expressive acts. If that were the case, then voters, depending on their perceptions of where they “belong,” might not seek to punish the poorly performing party or might not care if the party performed well (as it is the “other” party or not “my” party).

There is, in fact, behavioral evidence to suggest that this is what happens. Citizens’ evaluations of the economy often do not translate into a vote for or against the government because people do not necessarily attribute responsibility for economic conditions to the incumbent government. As it turns out, citizens are often motivated to make the wrong attribution. In particular, voters attribute problem-solving competency and responsibility for good performance to the party they support and blame opposite parties for inferior economic performance (Rudolph 2003; see also Peffley, Feldman, and Sigelman 1987; Peffley and Williams 1985; however, see also Basinger and Lavine 2005; Norpoth 2001).

Or suppose that voting is not based on self-interest (cf. Meehl 1977; Blais 2000). Instead, it may reflect cultural scripts that are enacted through ritual behavior in elections and designed to remind members of a political community of their shared values and shared communal responsibility for self-government (McLeod 1999). How would this shift in theoretical perspective affect the analyses? At a minimum, it would lead to different interpretations of the data and perhaps different normative implications of our findings. While I doubt that it would take a great deal of persuasion to convince comparative behavioral researchers that culture is “real,” and that it can act powerfully in ways that are similar to those of formal institutions or other structures, the key question is whether a different theoretical perspective produces a compelling alternative story that can be validated with solid data.

Despite these potential affinities between culturalists and behavioral researchers, and despite Almond and Verba’s and Inglehart’s pathbreaking work on culture and changing political values, culture remains the stepchild of comparative behavioral politics. On occasion, as in Putnam’s work on civic life in Italy, cultural categories are successfully employed as analytical tools by behavioral researchers, but by and large, these efforts remain the exception that proves the rule. To the extent that culture and changing values are part of the comparative mass politics paradigm, they enter via the back door of structure – that is to

say, because structure (in the sociological sense) appears to have become a less powerful determinant of action (Dalton 1984).

I suspect that part of the relative neglect of culture in mass politics research also has to do with the difficulty of communicating across the cultural-behavioral divide. And, to be fair, significant obstacles to a closer relationship do exist. While behavioral researchers, like culturalists, are interested in the subjective dimension of politics, they tend to be less focused on working out the mechanisms and tracing the processes by which culture works to constrain behavior. Part of it has to do with the technology of sample surveys, which do not easily capture intersubjective and cross-cultural meanings. Here, a critical obstacle is the problem of equivalence. The utility of cross-national surveys rises and falls with the ability to capture equivalent meanings across countries or individuals (van Deth 1998; King, Murray, Salomon, and Tandon 2003). To take some simple but vexing examples, what the terms “democracy,” “corruption,” or “human rights” mean, and whether they mean the same thing across countries and different people, is certainly debatable (and debated).

This need not be an obstacle to forging closer links between cultural and behavioral researchers. One relatively unexplored but potentially fruitful way of connecting culture, rationalism, and behaviorism is in how psychocultural interpretations (Ross 1993a) shape behavior. In particular, both behavioral and cultural researchers have an interest in capturing how people make sense of the world around them (see also Ross this volume). Part of this sense-making, in fact an essential first step in this process, is the formation of perceptions. And differentially situated individuals often form vastly different perceptions of the same phenomenon or fact: “The same factors that push actors to make sense of a situation also lead to cognitive and perceptual distortion, because the desire for certainty is often greater than the capacity for accuracy. Not only are disputants likely to make systematic errors in the ‘facts’ underlying interpretations, but the homogeneous nature of most social settings and cultural amplifiers reinforces these self-serving mistakes” (Ross 1997: 69). I doubt that most political psychologists would disagree with such an argument.

These systematic errors in perceptions and self-serving mistakes are easily recognizable in the work of behavioral researchers. For example, as mentioned earlier, people will see the state of the economy as they, perhaps unself-consciously, wish to see it or because of the self-reinforcing nature of homogeneous social environments (Huckfeldt, Johnson, and Sprague 2004). To take a different example, recent work in comparative behavior has sought to investigate the determinants of people’s perceptions of and attitudes about human rights, democracy, or corruption – often thought to be culturally laden and contested concepts – and the extent to which such perceptions are shared across and within countries (Anderson, Paskeviciute, Sandovici, and Tverdova 2005; Seligson 2006). As it turns out, there is significant heterogeneity across differently situated individuals, and this heterogeneity is often the result of cognitive biases.

To date, an important assumption underlying existing multilevel models concerns the stability of contextual features over time. Relaxing this assumption

could offer the opportunity of bridging the interests of culturalists, structuralists, and behavioralists in the endogeneity of institutions (see also Rodden this volume) and political processes generally. Institutions are hard to divorce from culture and can be thought of as reflecting culture (unless exogenously imposed). They also are endogenous to mass and elite behavior, and can thus be seen as being created and modified by (rational) strategic actors. While the studies of strategic voter behavior – as they examine the interaction between voters and electoral rules or the number of parties, for example – assume passive political parties, voters are clearly not the only actors when it comes to institutional change. It is plausible, if not likely, that parties also work to create preferences, during political campaigns as well as at other times (e.g., Farrell and Schmitt-Beck 2006). Political parties and governments are also heavily and often primarily involved in crafting new institutional solutions, and the fluidity or stability of structures – and concomitantly the exogeneity and endogeneity of institutions – is particularly likely to differ systematically in old versus new democracies, with the presumption of greater endogeneity or at least the potential for it in newer democracies. Alternatively, vote choice influences the number of parties (some win and do well; some do poorly or lose) and the parties' campaign appeals. All these processes interact so that vote choice, party behavior, and electoral rules are all potentially endogenous (see also Rodden this volume). The questions of institutional stability and endogeneity are thus critically related to basic assumptions in multilevel behavioral work.

If we think about comparative politics as a set of islands of study, as some have argued (Dalton 1991), the affinities discussed earlier suggest that the comparative study of mass politics is in the position of playing an important bridging role across theoretical and substantive islands. To use the language of social networks, behavioral politics is the approach that can fill structural holes in comparative politics by linking up with a variety of theoretical and substantive concerns, such as the study of democratic stability, mobilization, welfare states, or representation, to name just a few examples, via the connection of citizens' preferences and actions and macropolitical outcomes and vice versa.

Some have traveled down this path. In addition to the research on economic voting reviewed earlier, recent years have seen systematic investigations of the links between corruption and subjective outcomes such as regime legitimacy and happiness (Anderson and Tverdova 2005; Tavits forthcoming), political institutions and regime support (Anderson and Guillory 1997; Anderson, Blais, Bowler, Donovan, and Listhaug 2005), religion and preferences for social insurance (Scheve and Stasavage 2006), processes of domestic representation and support for European integration (Rohrshneider 2002), income inequality, trust in government, and voter turnout (Anderson and Singer 2008; Anderson and Beramendi 2008), and the link between welfare state institutions and policies to citizens' preferences and attitudes about work (Iversen and Soskice 2001; Anderson and Pontusson 2007), to name only a handful of examples (see Anderson 2007a for a more extensive review). The point is that students of

behavioral comparative politics have started to take full advantage of the new world of better theories, data, and multilevel statistical techniques.

But inherent in the good fortune of occupying, or potentially occupying, this central space in comparative politics is the risk of being stuck in the nowhere land of not being central to any one theoretical or substantive debate in different subfields. On balance, my own sense is that the evolution of comparative behavioral researchers toward ecumenism is fruitful and exciting. Instead of prizing one theoretical perspective – culture, structure, or rationality – over another, creative thievery allows the construction of an intellectual framework that is broad but theoretically grounded. What is more, given that much of political science is aimed at explaining behavior – the decisions taken by governments, political actors, groups, organizations, parties, and the like – behavioral politics is extremely well positioned to interrogate basic assumptions scholars make about how individuals come to make particular choices, under different conditions, in different contexts, and at different points in time:

Even where variables are not explicitly nested, they will be implicitly so in theory, as in questions about the relationship between democracy and economic development; though these are both macro-level variables, all arguments about their relationship involve assumptions about how various subsystem players (labor, capital, the military, etc.) interact under varying system-level conditions. Comparative Politics, dealing as it does with how politics operates in varying political systems, appears by its very nature to be multi-level. (Kedar and Shively 2005: 297)

SOME LAST WORDS: LOOKING FOR A GRAND THEORY OF MULTILEVEL POLITICS?

Comparative politics has long been dominated by macro and meso approaches. While countries and their cultures, structures, and institutions are obviously important and legitimate scholarly concerns, relatively few comparativists have focused on understanding the behavior of individuals as political actors. What is more, research focused at the macro level and research into the behavior of individuals for many years have existed in separate worlds. This is unfortunate, as people – in their roles as voters, citizens, consumers, union members, party leaders, activists, protestors, taxpayers, workers, parents, farmers, and policy-makers – are at the heart of politics. What they think and do (or do not do) is critical for understanding such central and oft-investigated phenomena as elections, revolutions, regime stability, legitimacy, political culture, ethnicity, corruption, redistribution, and welfare states – or any number of subjects that link citizens' desires and government action.

I have reviewed a number of reasons for these trends, but an important part of the story is that this situation has begun to change in recent years as a new generation of comparative researchers has started to take behavioral politics seriously and to develop ways to connect individual behavior and cognitions and scholarship in comparative politics more broadly. In contrast to much behavioral research in American politics, this research starts with the simple and perhaps

obvious recognition that people do not live in a vacuum.¹² Instead, they are nested citizens insofar as they form attitudes and make choices in variable environments – be they formal (institutional) rules, informal rules, such as norms of behavior and cultural scripts, or differential structural conditions. These environments vary systematically across countries or regions, and they are theorized to powerfully shape what people do and think. Put simply, the comparative study of mass politics is in the process of becoming the study of nested citizens.

The study of nested citizens is broadening the substantive repertoire of mass politics research from one focused on social structures and electoral behavior in the rich democracies to one that combines a (potential) multitude of macro-level factors and individual-level behaviors in most countries around the world. Thus, compared to only a decade ago, when Barnes (1997) was writing his review of mass politics, newer strands of research and availability of data have opened up the possibility of going far beyond the advanced industrialized societies that have traditionally constituted the core of scholarship on comparative mass politics, as well as going beyond the narrow focus on electoral behavior and conventional politics to include questions of legitimacy, values, and political economy – and, as a result, the possibility of occupying a central place in the study of comparative politics.

But this opportunity is not entirely risk-free. A particular risk lies in the indiscriminate and theoretically unreflective proliferation of studies examining macro–micro connections in the absence of an overarching theoretical framework. At the same time, such a framework is hard to construct. For example, modeling the interactive effects of structures and individual differences on behavioral outcomes and the analytical strategies underlying the examples of studies of voter behavior described earlier presumes that macro contexts are exogenous and static – or at least sufficiently so in the short term to allow for a systematic examination of structures and behavior.¹³ Needless to say, this is unlikely to be the case in the long run and less likely to be the case in some contexts, such as democratizing or rapidly changing societies.

This recent wave of comparative political behavior is rooted in long-standing research programs in comparative politics and mass politics through their focus on institutions or social structures. Studies of voter behavior that emphasize variations in structures (formal rules and/or political parties) also link easily to other substantive concerns prominent in comparative politics today, and especially to analyses of cabinet formation, the impact of government partisanship on policy outcomes, and our understanding of the role of elections and institutions of representation in shaping the quality of democracy. These concerns link to multilevel analyses in straightforward ways; in contrast, the single-country

¹² Notable exceptions in American politics include, for example, Huckfeldt (1986) and Huckfeldt and Sprague (1995).

¹³ Another way to think about this is to say that the relationship between citizen behavior and context is assumed to run from context to behavior and to be in equilibrium.

studies that are derived from the Michigan model are on the margins of comparative politics. As a result, the study of mass politics has become more central to the study of comparative politics, perhaps in unanticipated ways, through its ecumenical relations with different theoretical traditions in the social sciences and comparative politics.

In the end, it is important to welcome the current wave of multilevel comparative behavioral research that seeks to integrate what we know about macro-level politics, which comparative politics has long specialized in, with micro-level cognition and action. To be sure, behavioral politics, as currently practiced, is weak on process and mechanisms, but it explicitly recognizes that contexts matter, that they are variable across space and time, and that they are commonly the product of social choices human societies make. Moreover, they are seldom neutral in that they usually have differential costs and incentives for differently situated individuals. And because these environments have non-neutral consequences for different kinds of people, they provide individuals with incentives and conditions for viewing the world and behaving in particular and distinct ways.

Such a picture of the interaction between structures and behavior views politics as the interaction of people's values and the rules and conditions that govern the implementation of those values. These interactions occur at different levels of analysis, and they connect various elements of explanations in a complex causal chain with many moving parts. This world is fundamentally different from early studies of political behavior, which assume direct effects between rational calculations or self-interest; or culturally based preferences; or structurally derived preferences and citizen action. Thus, they take up Zuckerman's challenge (see Zuckerman this volume) to develop models that apply to "stochastic, multilevel, and reciprocal phenomena, not only simple one-directional causal claims."

A couple of years ago, when I was presenting some research I had done using a multilevel modeling framework, a colleague asked me whether I saw myself as a structuralist or a rationalist. At the time, I didn't quite know what to say. He told me that the answer was simple: He thought I was both, and in hindsight I think he was right. I would only add that I think cultural approaches and questions should be part of the agenda of multilevel behavioral politics as well. Such a perspective of citizen politics recognizes that the rules and realities in which citizens make choices are themselves a function of people's values (Riker 1980). If it takes this perspective, multilevel behavioral research is well positioned to connect the hitherto disconnected and allow behavioral researchers to engage with and be engaged by long-standing but unresolved debates in comparative politics.

Back to the Future

Endogenous Institutions and Comparative Politics

Jonathan Rodden

Like a fashion trend traveling from New York to the heartland, soul-searching about causality has made its way from empirical research in economics to that in political science with the usual lag. Gone are the days when it was enough to have a nice theory, a conditional correlation, and some rhetoric about the implausibility of competing explanations while implying but assiduously avoiding the “c” word. Editors, reviewers, and search committees are beginning to look for more explicit and careful empirical treatments of causality. That is, following a definition of causality tracing back to Mill (1848), researchers are expected to lay out a set of possible outcomes, or counterfactuals, generated by a set of determinants, and demonstrate that holding all possible determinants except one at a constant level, the manipulation of that determinant is associated with a specific change in outcome, which can be deemed a causal effect (Heckman 2005).

It does not take much soul-searching to realize, however, that the observational studies that make up the vast majority of empirical explorations in comparative politics are deeply flawed when held up to the experimental ideal. Where does this leave us? In the extreme view, if we cannot do randomized field experiments or perhaps survey experiments, we should do nothing. The opposite extreme position holds that this would remake comparative politics into an arid subfield of program evaluation, turning a blind eye to the interesting and important questions that animated the field in its golden era (definitions vary). In this view, there is a steep trade-off between “interesting” and “provable,” and the best way forward is to maintain a more casual approach to empirical explorations of causality given that the basic problems are intractable for the questions at the heart of the subfield. As one colleague put it, it is better for the field to suffer from omitted variables than omitted questions.

Rather than entering an abstract debate about the philosophy of science, this chapter opts for an admittedly rather offhand descriptive empiricism. How are empirical researchers in comparative politics grappling with problems of causality in their work? To put it bluntly: Is comparative politics getting less interesting? Does our newfound concern with causality turn us away from

Katzenstein's (1995) "important and interesting questions" (p. 11) or turn us into Eckstein's (1964) "dullards"? (See Lichbach this volume.) If we ignore all of the data generated by history save for the rare instances when it bestows natural experiments or the instances when human subjects review boards allow for truly randomized treatment, how much have we given up?

Moreover, are we making any progress? Or are we merely tilting at windmills, applying new techniques but continuing to fall short in our efforts to disentangle correlation from causality? Even worse, perhaps we are fooling ourselves by trading some dubious but straightforward assumptions about unit homogeneity and conditional mean independence for equally dubious but less transparent beliefs about instruments or propensity scores.

This chapter addresses these questions by focusing on endogenous democratic political institutions. One of the achievements of comparative politics over the past 50 years is that it has discovered some impressive associations between institutions and various political and economic outcomes, and many of these are consistent with rather attractive informal and formal theoretical models focusing on the ways different institutions shape incentives for voters, interest groups, and policymakers. But there has always been an elephant in the room that is only sporadically acknowledged: The institutions (e.g., democracy itself, federalism, electoral rules) are themselves endogenous, and we know relatively little about the processes by which history assigns countries to institutional categories. It is not implausible that some of our most cherished "findings" are epiphenomenal. Perhaps there is some unobserved historical process that generates both democracy and development. Perhaps some dimly understood cultural characteristics or some aspect of British colonialism pushed some countries to maintain majoritarian electoral systems and fewer political parties, while some other process drove continental Europe and Latin America toward proportional representation and multiple parties. Or perhaps, if parties are key actors in choosing electoral systems, Duverger's law is turned on its head and electoral rules are driven by party systems.

This class of problems is at the center of the current research agenda in comparative politics and political economics. Since the 1960s, comparativists have been building theories linking institutional rules with various political and economic outcomes and examining them with cross-country data sets – first limited to the Organization for Economic Cooperation and Development (OECD) and now much larger. Informal theories like those of Duverger (1964) and Lijphart (1999) have been supplemented with formal theories like those of Cox (1997) and Persson and Tabellini (2000). As the theories become more precise, empirical studies are able to move beyond the blunt cross-country correlations and hone in on the causal mechanisms they imply. And in the past few years, there has been a concerted effort to confront the pitfalls of causal inference given that institutions are not randomly assigned.

In order to stay focused, this chapter follows these developments in a relatively recent literature that links electoral rules with the generosity of the welfare state. A fairly stubborn stylized fact has been established, and a variety of alternative causal mechanisms have been proposed. The next step – still in

progress – is to link the specific mechanisms with appropriate empirical tests. But the most difficult challenge has been to assess whether electoral rules might plausibly cause distinct public policy outcomes at all, given the myriad unobservable factors that might drive the selection of both electoral systems and public policy outcomes. Researchers are only beginning to examine this question. First, they have used the standard econometric techniques, like selection models and instrumental variables. Second, they have delved into the question of electoral regime choice with a mixture of theory and the analysis of quantitative and qualitative data drawn from the nineteenth century to the present. Third, they have sought out natural experiments, and finally, they are beginning to conduct field experiments.

After critically reviewing this evolution, I broaden the scope and note that the literatures on the effects of other institutions – in particular federalism and democracy itself – have followed a similar trajectory.

In response to the questions posed earlier, I argue that progress in clarifying the causal pathways linking institutions and outcomes is slow, difficult, and uneven but palpable and worth the effort. Moreover, the shift of attention to matters of causality in comparative politics has done anything but make the field less interesting. On the contrary, it has breathed new life into a set of fascinating basic questions about politics and history. The study of institutional origins has become one of the most vibrant research areas in comparative politics. This is a research field in which structural, rationalist, and, to a lesser extent, cultural approaches collide and interact.

I will argue that it constitutes one of Eckstein's (1980) "core difficulties": Further progress in the study of institutions cannot take place until this problem is addressed. It also accords with Lichbach's (this volume) description of a "thorny puzzle." Yet, researchers are also motivated by what he refers to as "big problems" in the real world. Political scientists have immersed themselves in institutional design and reform debates in countries conquered by the United States, as well as at the World Bank, the International Monetary Fund (IMF), and the other development banks. But unless they specify convincing theories about the conditions under which specific institutions emerge, evolve, and stabilize, they are selling snake oil.

I conclude that even if they ultimately fall short, comparative social scientists have no choice but to set high standards for themselves in the empirical demonstration of causality. This requires attention not only to empirical methodology, but to theory-building as well. For the questions that seem most important, everything seems to be endogenous to everything, but theory helps cut through the web to develop identification strategies. Given the rapid rise of experimental research in political science, observational researchers risk being marginalized if they are not more honest about the specifics of the biases inherent in their work and do not explore every possible technique for limiting them. The only other alternative to a stubborn obsolescence seems to be a "barefoot empiricist" insistence on exclusively experimental research that many comparativists would find unattractive.

STARTING WITH A STYLIZED FACT: ELECTORAL RULES AND THE WELFARE STATE

Countries using proportional representation (PR) appear to have significantly larger welfare states and conduct more redistribution than those using majoritarian electoral rules. Though the measurement of the dependent variable is controversial, and requires researchers to subjectively code line items in government budgets as “social” or “welfare” expenditures, the relationship seems to hold up using data collected by the OECD as well as the IMF, in addition to a less subjective measure capturing the difference between income inequality before and after taxes and transfers drawn from the Luxembourg Income Study. The independent variable has been conceived in various ways: generally either as a categorical variable or as a continuous measure of the proportionality through which votes translate into seats. While most studies focus exclusively on wealthy countries (Crepaz 1998; Lijphart 1999; Huber, Ragin, and Stephens 1993; Iversen and Soskice 2006), the relationship holds up in Persson and Tabellini’s (2003) sample of 60 countries, and appears to hold up in multivariate ordinary least squares (OLS) regressions that include all of the usual covariates in cross-country welfare expenditure models.

Other policy differences have been attributed to electoral rules as well (see the later discussion), but it is useful to focus on the welfare state because it has received by far the most empirical attention from sociologists, political scientists, and economists. Moreover, researchers have developed several distinct causal mechanisms, each with its own empirical implications and endogeneity challenges. Let us assume that these scholars are on to something and examine the progress they’ve made in their efforts to sort out causality.

CAUSAL MECHANISMS

Contributors to this literature vary in the hubris of their claims to have unambiguously demonstrated causality, but a causal role for electoral institutions is usually at least implied. Iversen and Soskice (2006) described their empirical results with language like “affect” and “is a result of” but generally avoided “cause,” and then promptly published another paper (Cusack, Iversen, and Soskice 2007) implying that electoral rules are endogenous and at best an intervening variable. Persson and Tabellini (2003), on the other hand, were quite explicit in claiming that electoral rules cause policy outcomes.

Before examining the thorny endogeneity problems, it is useful to clarify the causal mechanisms suggested in the literature. I break the causal claims into three broad categories.¹ First, electoral rules might shape politicians’ incentives to provide specific versus targeted benefits. Second, electoral rules might shape policy by favoring parties of the left or right in the process of government formation after elections, or even before that stage in the translation of votes to

¹ For a more complete literature review, see McDonald (2006).

seats. Third, in the context of a simple spatial model, electoral rules might shape party platforms, and hence policies, by shaping the identity of the relevant median voter either because of geographic clustering or an impact on turnout.

Persson and Tabellini's (2000) model has quickly become the dominant perspective. The logic is simple. With majoritarian rules and single-member districts (SMDs), a party need only receive a plurality of votes in half of the districts plus one in order to win an election and form a government. This provides incentives for reelection-seeking incumbents to target expenditures and other policy benefits to the pivotal districts that determine election outcomes while ignoring superfluous districts. Under PR (in the ideal case, with one national district), a vote is worth seeking no matter where it is located, and parties consider the welfare of the entire electorate as they attempt to maximize their representation. This provides them with incentives to propose and implement policies, like the welfare state, that are national in scope. Different modeling choices and assumptions generate theoretical results with a similar flavor in Lizzeri and Persico (2001) and Milesi-Feretti et al. (2002).

Iversen and Soskice (2006) envision an entirely different causal path. Their starting point is a party's preelection inability to commit to the policies it will pursue after the election. In a system with single-member districts and only two political parties, middle-income voters do not want to risk choosing the party of the left – even if it claims to have moderate positions – for fear that it will succumb to its left wing after the election and tax both the rich and middle class in order to redistribute to the poor. The worst that can happen to the middle class if the party of the right reneges on its moderate promises is that public services fall below their desired level. Under PR, the middle class is represented by a centrist party that can bargain with parties on the left by extracting a tax-transfer scheme that soaks the rich, and the party of the left is in a better position to form coalitions with the center than the right. Iversen and Soskice (2006) summarize evidence that left-center governments are far more common under PR than under majoritarian rules in OECD countries, and argue that the causal impact of institutions on redistributive policy flows through a partisan bias in the coalition-formation process.

Building on observations made by Butler (1951) and Gudgin and Taylor (1979), Rodden (2006) argues that the geography of industrialization is such that leftist voters are highly concentrated in cities and mining areas. Thus, it is difficult to draw small SMDs in industrialized societies that are not biased in favor of the right. Indeed, it appears that a bad geographic distribution of support has been responsible for a systematic electoral bias against the right in OECD countries using majoritarian electoral rules since World War II. Moreover, there is an asymmetric distribution of district-level vote shares such that there are far more districts (typically in large cities) dominated by the left than by the right.

It seems at first glance that this bias might not impact policies in the long run, since instances in which right-wing parties form governments while receiving fewer votes than their leftist competitors are relatively rare. However, the

asymmetric distribution of district-level vote shares (and preferences over policies) might induce a more subtle effect. Either because they fear entry by new parties on their left flank in the cities or because urban leftist incumbents dominate the choice of electoral platforms, parties of the left under SMD might find it difficult to offer a platform that is sufficiently moderate for the median voter in the median district. Thus, the left might lose relatively more often under SMD because their platform is too extreme for voters in the crucial districts. Or even if the party of the left successfully staves off its urban base and moves its policy position to that preferred by the median voter in the median district, given the asymmetric distribution of district medians, this policy will be to the right of that preferred by the median voter in the country as a whole, thus tilting policies to the right under SMD relative to PR.

Finally, it is possible that electoral rules cause policy differences by altering the preferences of the median voter in a different way. Countries using PR have long been known to demonstrate higher levels of electoral participation. Higher turnout is often thought to imply greater participation among the poor, which should move the preferences of the median voter leftward, with obvious implications for social policy (Franzese 2002; Boix 2003).

EMPIRICAL ATTEMPTS TO ILLUMINATE MECHANISMS

An attractive feature of this literature is that each causal mechanism contains implications beyond a simple correlation between electoral rules and welfare expenditures. For starters, the authors have sought to differentiate their products with different ways of measuring the dependent variable. Iversen and Soskice (2006) note that their model is about income redistribution, and correctly point out that many “welfare” expenditures in Europe are not redistributive at all, but are in fact subsidies for the middle class, leading them to use data from the Luxembourg Income Study to capture redistribution. This is not a problem for the Persson–Tabellini model described earlier, since their aim is to use social expenditures not to capture redistribution, but rather the prevalence of programs that are national in scope. Milesi-Ferretti et al. (2002) have a similar goal but choose a different set of line items from the same budget data.

Ultimately, these efforts don’t help differentiate between the causal mechanisms, though, since these variables are highly correlated, and all seem to demonstrate the same pattern. In fact, the relationship looks fairly similar when overall expenditures as a share of gross domestic product (GDP) are used as the dependent variable – a relationship that Persson and Tabellini (2003) attribute to a different causal mechanism, subsequently updated by Persson, Roland, and Tabellini (2007). It is worth mentioning that PR is also said to be associated with a host of additional policy outcomes, including but not limited to higher income taxes (Powell 1982), higher deficits and different ways of adjusting to macro-economic shocks (Persson and Tabellini 2003), more liberal abortion policies (Powell 1982), greater efforts at environmental protection (Lijphart 1999; Fredriksson and Millimet 2004), lower incarceration rates and less use of the

death penalty (Lijphart 1999), and policies favoring producers over consumers (Rogowski and Kayser 2002). Most of these seem to be policies favored by the political left, though in some cases, one might also tell stories about general versus specific interests (e.g., Fredriksson and Millimet 2004 on environmental protection).

Another way to bolster or undermine confidence in the various mechanisms would be to find cross-country indicators that capture one causal claim or another and use them as independent variables, subjecting them to various statistical horse races. For instance, one might try to show that the causal impact of electoral rules runs through such readily observable things as partisanship, coalition government, bias in coalition formation, bias in the vote-seat curve, or turnout. To my knowledge, this has not yet been done with a large data set, though in Iversen and Soskice's limited OECD data, it appears that the impact of electoral rules on redistribution is not attenuated very much when controls for turnout, partisanship, and electoral bias are added to the model.² Perhaps this should not be surprising, though, since the causal mechanisms reviewed earlier are not mutually exclusive – all could be contributing to the correlation.

Perhaps a more promising way to hone in on the causal mechanisms is to move beyond cross-country regressions. For instance, the dominant perspective implies that under SMD, public expenditures should be targeted at swing districts, while the “core support” districts for the two parties should receive far less. It so happens that there is a huge empirical literature on this question, though the theoretical motivation is generally a contrast between Cox and McCubbins (1986) and Dixit and Londregan (1995). It is difficult to summarize the literature without conducting some kind of meta-analysis, but my impression is that the strong version of the “swing district” logic that drives the Persson–Tabellini model has not fared well in empirical tests (see, e.g., Ansolabehere and Snyder 2006).

It is of crucial importance for Iversen and Soskice (2006) that there is substantial bias in the process of coalition formation under PR. That is, over a long period of time, cabinets tend to be to the left of legislatures. Admirably, they seek to demonstrate this by contrasting the ideology of the median cabinet member with that of the median legislator, using the average of several “expert survey” scores of party ideology as their proxy. They find a gap indicative of left bias. Unfortunately, however, this has been called into question by McDonald, Mendes, and Budge (2004), who find no such gap when using data from party manifestos rather than expert surveys as their proxy for ideology. Even if one trusts the “experts” more than the coding techniques used in the manifesto project, it would be useful to dig deeper and verify that the bias comes from

² Using data for total expenditures, Boix (2003) finds that turnout washes out the impact of electoral rules. Also using data for total expenditures, Persson, Roland, and Tabellini (2007) claim to show, using a two-stage model, that the causal role of electoral rules is in shaping the number of parties, which in turn shapes the prevalence of coalition governments, which spend more than single-party governments.

center parties that, as *formateurs*, choose to eschew equally attractive parties on the right in favor of the left. Moreover, it would be useful to demonstrate that in PR systems, there is indeed such an animal as “middle-class” parties whose voting support comes primarily from middle-income voters. An alternative possibility is that multiparty systems under PR reflect the presence of multiple issue dimensions rather than a set of finer-grained distinctions along a single economic dimension. Finally, Iversen and Soskice (2006) have generated a fascinating, intuitively appealing, yet to my knowledge untested hypothesis that might be amenable to research using survey experiments: When there are two parties, risk-averse middle-class voters are more fearful of the left than the right, even if the two parties offer the same platform.

Rodden (2006) finds evidence that industrial geography creates the hypothesized asymmetric distribution of district-level vote shares in several countries with majoritarian electoral rules, and demonstrates bias in the translation of votes to seats, but does not (yet) connect any of this with public policy outcomes. An important task in unearthing the causal mechanism here is to get estimates for district-level preferences that are not drawn from election results in order to examine whether in fact there is a gap, as hypothesized, between the preference of the median voter in the country and the median voter in the median district.

Finally, the causal mechanisms suggested by the turnout hypothesis are also a good target for careful empirical research. Do exogenous increases in turnout lead to greater participation among the poor? If so, does this in fact shift the preferences of the median voter? Moreover, it would be nice to know if the lower turnout rates under SMD are merely a product of voter apathy in the “safe seats” that figure so prominently in Persson and Tabellini (2000) and Rodden (2006). It would be straightforward to match on some observable district characteristics and contrast turnout rates in the most competitive districts of SMD systems with those in PR systems.

THE ENDOGENEITY PROBLEM

Hopefully, the forgoing discussion conveyed the sense that the exploration of causal mechanisms linking electoral rules and public policy is an active and interesting research program. However, I have not yet mentioned the elephant in the corner. These mechanisms are not causal if they are driven by unobserved characteristics of countries that also drive welfare expenditures and redistribution. Rather than attempting a review of causal inference problems in comparative empirical research in the presence of simultaneity, I direct the reader to Heckman (2005) or Przeworski (2007) and the citations therein.

The basic notion of causality implied in most of the literature reviewed earlier is explicitly addressed in Persson and Tabellini (2003: 114): “We would thus like to answer counterfactual questions like the following: Suppose we pick a country at random in our sample and, going back in history, change its constitution. How would this alter its current performance?” The argument is that if it were possible to randomly assign the “treatment” of PR to one group of countries and

SMD to another, the PR countries would develop larger welfare states (along with greater attention to national public goods, left bias, higher turnout, and so on, depending on the theory). The causal effect of the treatment on an individual country is defined as the difference between the outcome variable in that country under PR and SMD, and we are interested in the average of those individual treatment effects.

The problem, of course, is that we cannot observe the relevant counterfactual. For the most part, in the era of the modern welfare state, we are only able to observe Sweden under PR and the United Kingdom under SMD (more on this later). Thus, researchers implicitly make the assumption of unit homogeneity (Holland 1986), which states that if two units have the same values of covariates, they would demonstrate the same states under control and the same states under treatment. The problem of selection is assumed away, and it does not matter which of two units goes into the control group and which into the treatment group. This must be assumed rather than tested, but the assumption seems reasonable enough when the investigator can randomly assign individuals to the treatment and control groups.

Additionally, the studies reviewed earlier implicitly assume conditional mean independence. That is, the countries that have not been exposed to the treatment (PR) would (hypothetically) react to it identically to those where the treatment has been received, and the countries receiving the treatment would (hypothetically) not differ in their control state from the group in which the control state (SMD) is observed. Again, this is achieved in experimental studies through randomization.

But in studying national constitutions, of course, researchers rarely have opportunities for random assignment.

Thus, the literature on institutions and outcomes is based on very strong assumptions. To the extent that they are not met, the inferences about the causal impact of institutions are biased. While reverse causation and measurement error might also be important, the most obvious problem here is *selection*. First of all, there may be an unobserved difference in the control state between the countries that were exposed to the treatment and those that were not. It is possible, for instance, that PR emerged in countries like Sweden, where a “history of equal opportunity, broad education, and widespread ownership of land . . . fostered a common culture of equality that has promoted the selection of a proportional electoral system as well as a preference for a welfare state” (Persson and Tabellini 2003: 115). The problem here is that such a “culture” and its associated “preferences,” especially to the extent that they are rooted in the nineteenth century or earlier, are difficult to observe and measure across countries. Moreover, British colonial experience is observable, but it is so highly correlated with electoral rules in OECD data sets that any separate impact of electoral rules would be driven by France and Japan alone.

Given the preceding arguments about partisan bias, one can also envision a self-selection bias, where the *effect* of the treatment on the treated countries is different from its (unobserved, hypothetical) effect on those who did not receive

the treatment. This might happen if, for reasons unobserved by the researcher, countries recruit themselves into the treatment group because they anticipate a desired outcome. For instance, anticipating greater redistribution, the poor and vulnerable might push for PR. Alternatively, if they are aware of the logic sketched by Iversen and Soskice (2006) or Rodden (2006), party leaders on the left should advocate for PR. If the poor, the leftist parties, or both, are more mobilized and powerful in some countries than others, they may be able to maintain their favored institutional framework along with their favored policies. In this event, electoral rules should not be viewed as causing policy outcomes.

EMPIRICAL STRATEGIES TO CONFRONT ENDOGENEITY

Empirical researchers have developed (at least) four strategies for dealing with these problems. First, they reach into an ever-expanding econometric toolkit relying on structural models. Second, these structural models call for the analysis of history from a new perspective. Third, researchers attempt to exploit natural experiments. Fourth, they are beginning to conduct field experiments.

Econometric Analysis

In their path-breaking book, Persson and Tabellini (2003) devote considerable attention to exactly this class of problems. After finding a robust relationship between electoral rules and expenditures in a multivariate OLS model, they move on to explore matching estimators. As with OLS, this approach assumes that selection of countries into constitutional rules is essentially random, and using a matrix of observable attributes, researchers develop a propensity score that aims to match each country using a particular constitutional rule with an otherwise similar country using the opposite rule. While this is a potentially useful innovation given concerns about nonlinearities in the relationship between constitutions, observable covariates, and outcomes, it still sweeps the endogeneity problem under the rug by assuming conditional independence.

Like most other comparative institutional researchers, Persson and Tabellini rely primarily on the instrumental variables strategy to guard against the biases that might arise if conditional independence and unit homogeneity do not hold. The strategy is to find a vector of instruments that predicts the endogenous regressor (electoral rules) but is orthogonal to the error term, which means that the vector of instruments has no effect on the size of the welfare state other than its impact through electoral rules.³

Finding good instruments for institutions like electoral rules is surely more art than science. First, the instruments must be highly correlated with the institution in the first stage regression in order to avoid a cure that is worse than

³ They also use Heckman selection models, but since these rely on the same exclusion restrictions as the instrumental variables approach, much of the discussion that follows is relevant for this approach as well.

the disease (an even more biased estimator than that caused by the selection problem). Second, the instruments must be exogenous. In this context, controlling for the set of instruments, the remaining random component of constitutional selection must not be correlated with the country-specific, unobserved change in welfare expenditures associated with a change in electoral rules. Sadly, this can be asserted but not tested. Thus, the researcher must tell a convincing story about the exogeneity of the prized instrument, but a leap of faith is always required.

Persson and Tabellini (2003) spill surprisingly little ink convincing the reader to take this leap and have little to say about the justification of their instruments. First, they include three variables that place the year of adoption of the country's constitution into four periods. The idea is that there appear to have been "waves" during which some constitutional types were popular, and the period during which a country declared independence or adopted its constitution might be correlated with the electoral rules, but Persson and Tabellini argue that there is no reason to believe that the pure timing of constitutional choice should have an impact on contemporary expenditure outcomes. This might be debatable since, as discussed later, many of the countries with the largest welfare states selected institutions of PR during a very brief period after World War I when soldiers returned from the war, the franchise was expanded, and socialist parties were mobilizing – sometimes in the streets and with weapons. In any case, such a debate may be irrelevant for their estimation, because in a reanalysis of their data, Acemoglu (2005) shows that these instruments are hopelessly weak, and do not approach statistical significance by themselves in the first-stage electoral rule equation.

Thus, the identification strategy relies solely on the additional instruments, which were adopted from the work of Hall and Jones (1999): latitude, the percentage of the population whose native language is English, and that for whom it is another European language. Though Persson and Tabellini are largely silent about the logic behind these instruments, the idea is apparently that these variables reflect the depth of European cultural influence on clusters of institutions⁴ but do not have a direct impact on contemporary fiscal outcomes.

The coefficients in the first-stage regressions suggest that, indeed, "proportion English speaking" is associated with majoritarian electoral rules, while "proportion speaking other European languages" is associated with PR. Yet, the English-speaking variable is essentially a proxy for the British colonial experience, and it is not at all clear why one should believe that electoral rules are the only, or even the most important, aspect of British colonialism that might impact welfare expenditures. For instance, according to La Porta et al. (1998), these countries had more developed financial markets, which may have had an impact on the development of government spending. One can think of many

⁴ In the literature from which these instruments are drawn, these institutional clusters are understood as things like "property rights" and "rule of law" rather than specific institutions like electoral rules.

additional unobserved aspects of British, or for that matter French, Belgian, or Portuguese colonial experience that might have an impact on long-run patterns of government expenditure. The Hall and Jones instruments are best suited to a sample of colonies, but many of the countries in the Persson and Tabellini sample in which European languages are spoken are located in Europe, of all places, and here again, there are many unmeasured aspects of “Europeanness” – for example, culture, preferences, the power of labor unions and the left – that might have an impact on welfare expenditures that does not flow through electoral rules. Acemoglu (2005) makes the important point that even if a matrix of instruments seems plausibly valid for a cluster of interrelated institutions, it is probably not valid for a specific institution like electoral rules if the other associated institutions (common or civil law, bicameralism, etc.) are not entering the equation as control variables.

The point of this discussion is not that Persson and Tabellini have done shoddy work. On the contrary, these results and their connection to the underlying theories established in Persson and Tabellini (2000) represent one of the most important contributions to comparative politics in recent decades. There is clearly a relationship between electoral rules and government expenditures, and it holds up in a wide range of countries. Yet, in spite of their strong claims, the issue of causality is far from settled. It relies on exclusion restrictions – ultimately assertions – that are plausible but open for debate.

The same can be said for almost every other attempt to find historical instruments for institutions. Acemoglu (2005) sharply criticizes the validity of the Hall and Jones instruments, and the validity of even the most celebrated instrument for institutions – the settler mortality data mobilized by Acemoglu, Johnson, and Robinson (2002) – has been called into question (Djankov et al. 2003; Glaeser et al. 2004; Przeworski 2004a).

The point is not that the instrumental variables approach holds no promise. Rather, students of endogenous institutions must go well beyond searching for a simple, off-the-shelf magic bullet. Rather, as Acemoglu, Johnson, and Robinson (2005) argue, “To find an instrument one needs a theory of why institutions differ. The theory can act as a guide to find an instrument.” Instrumental variables and selection models are not technical fixes that allow researchers to avoid the hard work of delving into serious, theory-guided historical analysis. Rather, in order to come up with believable exclusion restrictions, one must essentially become an analytical historian. This requires more than a passing glance at the secondary literature.

In addition to searching for instruments, such theory-guided historical research might reveal that things previously deemed “unobservable” – like preferences of voters, the mobilization of labor unions and the left, social arrangements associated with the guild system, the strategic choices of elites – can indeed be theorized and measured. In other words, theory-guided historical research mobilizing quantitative and qualitative data can attempt to build more complex but satisfying structural models where institutions are chosen in the first stage and have an impact (or not) on contemporary outcomes in the second stage.

In short, it will be difficult to make progress in the comparative institutional literature without doing analytical history. Fortunately, this is exactly where the literature is headed.

Analytical History

Recent years have seen a renaissance in the study of endogenous electoral systems. Most of the analysis focuses on a handful of OECD countries, and much of the empirical action takes place in interwar Europe, while a more recent literature expands the time frame and cases, with special emphasis on the new democracies of Central and Eastern Europe. The first step in this renaissance was the publication of Boix (1999), which revived the classic argument of Braunias (1932) and Rokkan (1970) about electoral choice at the beginning of the twentieth century. Especially after World War I, the established elite parties understood that they could not avoid an expansion of the franchise, which they expected would dramatically increase the representation of a new, unified leftist party. When the old parties (often built around a town–rural cleavage) feared that it would be too difficult to overcome their differences and coordinate under SMDs, their best response was to choose PR in hopes of forming a postelection coalition. In the Rokkan and Boix stories, elites are apparently either unaware of the long-term disadvantages of PR for their class interests or are simply too concerned with their short-term electoral prospects. Rokkan describes right-wing parties that explicitly undermine their class interests and favor PR primarily because they fear being squeezed out of electoral politics by the other right-wing rival. In the terminology used in this volume, this is not a structural argument where social groups and classes play a role. The actors are self-interested politicians seeking to win the next election.

If Rokkan and Boix have it right, perhaps the problem of identifying subsequent causal effects of electoral institutions is not so severe. Perhaps some measure of partisan fragmentation on the right could serve as an instrument if one believes that these rifts on the right – many of them based on religious or town–rural cleavages that subsequently vanished – are essentially random and not plausibly correlated with long-term expenditure patterns.

But no such luck. The next thing to happen in the new literature on endogenous electoral rules was a rather intense focus on criticizing this theory and especially the empirical evidence. The Boix data have been reanalyzed by Cusack, Iversen, and Soskice (2007), Blais, Dobrzynska, and Indridason (2004), and Andrews and Jackman (2005), each of whom attacks the empirical results from a different angle and finds a way to make them disappear. Perhaps this is not surprising since these studies are based on *N* somewhere between 12 and 20 countries, but one critique in particular seems especially compelling. Blais et al. (2004) point out that the move to PR was generally not from the SMD plurality systems envisioned in the classic argument, but with only the exceptions of Sweden and Denmark, from systems with some form of two-round majority elections. Thus, it is difficult to understand why the right would have faced a

coordination problem, since, even if split in the first round, their voters should have had no problem coordinating on the sole remaining right-wing candidate in the second round. Moreover, it appears that parties of the right were not often in direct competition at the district level in the first place. In fact, most of these countries already had multiparty legislatures and coalition governments prior to the adoption of PR.

Thus, Blais et al. (2004) argue that the transition to PR was, in countries with multi-round elections, a fairly uncontroversial move spurred by intense public support for the PR trend that was sweeping Europe as part of a larger trend of democratization. Perhaps runoff SMD parliamentary elections, if kept in place, would have generated coalition governments not unlike those witnessed under PR. If this view is correct, for researchers interested in identifying a causal role for institutions, the relevant question is whether there is some reason to believe that there was something unique about countries with multi-round elections that made them more likely to develop large welfare states in the future. To my knowledge, this question has not been addressed.

Blais et al. (2004) seem to suggest that the enthusiasm for PR was equal across the political spectrum, and that the only skeptics were change-averse sitting parliamentarians who had gained their seats through the old rules. Again, the interests of class groups are not discussed. Like the traditional Braunias–Rokkan perspective, this almost certainly underestimates the affinity of the labor movement and the political left for PR. The adoption of PR was a key demand in the platform of virtually every socialist or workers' party at this time.

This point is emphasized by Alesina and Glaeser (2004), who argue that PR emerged in early-twentieth-century Europe as a direct response to the demands of a mobilized, revolutionary left. Their tour of early-twentieth-century Europe includes a brief paragraph on each country emphasizing the role of leftist movements in pushing for, and ultimately achieving, the expansion of the franchise to the poor and the adoption of PR through strikes, street protests, and the threat of violence. In some cases – interwar Germany, Austria, and Italy in particular – the aristocratic right was severely weakened by military defeat, and the maintenance of public order was in question. In some cases, like Weimar Germany, the left adopted PR after coming to power. In other cases, like Belgium and Sweden, Alesina and Glaeser suggest that even though PR was implemented by a party of the right, it was the implicit threat of violent revolution that forced their hand.

The treatment by Alesina and Glaeser may overstate the case and oversimplify the complex negotiations leading to electoral reform in the early twentieth century. In most countries, the left was pushing simultaneously to abolish property requirements for voting, reserved seats for business, plural voting (upper-class voters receiving tens and even hundreds of votes), and powerful appointed upper chambers representing the interests of the elites. PR was also on the wish list, but the expansion of the franchise was far more important. In Belgium and Sweden, the right-wing parties that made the initial transition to PR did so in an explicit attempt to throw bones to the left and quell the demand

for electoral reform while preserving their advantages through franchise restrictions, plural voting, and an undemocratic upper chamber (in Sweden) while maintaining small, badly apportioned districts that prevented proportional election results. In both countries, the initial transition to PR was essentially a sham (Carstairs 1980; Verney 1957). Yet, in spite of remaining franchise restrictions, these electoral reforms did strengthen the parliamentary representation of the left, and in the decades that followed, they were able to achieve their larger agenda to expand the franchise through a combination of parliamentary power and extraparliamentary agitation.

While the details are sparse, Alesina and Glaeser are clearly on to something. Socialist party leaders understood that PR was in their interest, and since the publication of Braunias (1932), the literature has lost sight of this by focusing on the complex handful of cases where right-wing parties disingenuously promoted PR. And as Alesina and Glaeser point out, this creates an identification problem for the literature positing a direct link from electoral rules to the rise of the welfare state later in the century. A precondition for PR was a strong, organized leftist workers' movement with organizational support from labor unions, which is also likely an important part of the story in the rise of the welfare state. According to Alesina and Glaeser, for various reasons the United States and Britain never developed this type of leftist organization and, as a result, developed neither PR nor a large welfare state. Electoral rules in this story are epiphenomenal.

Ticchi and Vindigni (2005) present a model with a similar flavor. Rather than focusing on electoral rules, they draw on Lijphart (1999) and contrast "majoritarian" with "consensus" democracy. In their interpretation of majoritarian democracy, there is a winner-take-all election in one large national district. There are three income groups/parties, and while taxes are proportional, expenditures are highly targeted to the group in power. Thus, the wealthy group dominates under majoritarianism because of its preference for low taxes, a phenomenon they refer to as the "dictatorship of the rich." They model consensus democracy as a process of coalition bargaining between the three groups and find that a less skewed income distribution facilitates a coalition between the poor and the middle class, which increases the size of government, while a more skewed distribution pulls together the rich and the middle class, putting the brakes on government expenditures.

Then, moving back a step, they examine the initial selection of institutions, which are chosen by simple majority voting, and every actor "correctly anticipates what their [sic] level of utility would be under the two possible constitutions, and vote consequently" (Ticchi and Vindigni 2005: 19). Their model implies that the majority in highly unequal societies would choose majoritarian rules. In a relatively equal society, the majority would choose consensus rules. The rather unusual feature of this model is that the poor rather than the middle class are the swing actors. The model implies that if income inequality is sufficiently high, the poor would be frozen out of a coalition-formation process dominated by the center-right under consensus democracy, which would cost

them more in taxes than the dictatorship of the rich but would still provide them with no public goods. Thus, for Ticchi and Vindigni (2005), each country's initial income distribution occupies the role played by the organizational power of the left in Alesina and Glaeser's story. It is an exogenous variable that drives both institutional choice and later public spending outcomes.⁵ Constitutional rules are once again epiphenomenal.

The same is true for Cusack, Iversen, and Soskice (2007). For them, the omitted variable driving both institutional choice and the development of the welfare state in OECD countries is the structure of the economy in the late nineteenth century. In this view, everything flows from exogenous "varieties of capitalism." They downplay the role of leftist agitation emphasized by Alesina and Glaeser (2004) and seem to build a theory around the cases of Denmark and the Netherlands, where PR was chosen peacefully and with little controversy as part of a cross-party bargain. As in the traditional Brauns-Rokkan-Boix perspective, PR is a conscious choice by the economic elites, who saw it as in their long-term self-interest. PR reflected a class compromise between employers and skilled workers in export-oriented economies who had a common interest in regulatory and social insurance systems that would protect investments in cospecific assets. In this theory, proportional electoral representation of parties in the legislature seems to be less important than a concomitant system of legislative committees representing employers and workers. Due to their lack of economic coordination or investment in cospecific assets, Britain and its former colonies avoided PR because the economic elites had no incentives for class compromise. If this theory is correct, we are again unable to identify a causal role for electoral institutions in the rise of the welfare state, since a history of local economic coordination through guilds drives the class compromise that gave rise to both PR and the welfare state.

Though quite different, the stories of Alesina and Glaeser (2004), Ticchi and Vindigni (2005), and Cusack et al. (2007) have some features in common. First, while the actors in the Brauns-Rokkan-Boix framework are rational, strategic, self-interested politicians who are primarily interested in getting and retaining power, possibly to the detriment of their long-term class interests, in these new models of endogenous institutions the actors are social classes, income groups, or social groups like factory owners and industrial laborers (farmers are curiously missing). Political parties are mere stand-ins for the interests of these groups. In the terminology of this volume, these works combine elements of rational choice theory and classic structuralism.

While the citizen-candidate framework of Ticchi and Vindigni (2005) is most explicit about it, each of these works assumes away collective action and agency problems that might be faced by actors on both the right and left. Potential

⁵ One should not characterize Ticchi and Vindigni's model as explaining redistribution, however, since redistribution across income groups is explicitly ruled out in the model, which makes very strict and unusual assumptions about targeting of expenditures to class groups and the impossibility of broad social programs.

conflicts between the old parties of the right are ignored, as are the serious rifts between Communist revolutionaries and Social Democrats on the left, and the fact that within parties, individually rational behavior does not translate into social rationality. In general, this literature can be described with the same words used by Przeworski (1985: 382) to describe Marxism: “What was important about history happened at the level of forces, structures, collectivities, and constraints, not individuals. . . . Marxism was a theory of history without any theory about the actions of people who made this history.”

Second, these class or income groups are farsighted and have full information about the long-term impact of their choices. There is no veil of ignorance. For Alesina and Glaeser (2004), who discuss endogenous electoral rules directly after presenting some regressions establishing the relationship between electoral rules and social expenditures, it is self-evident that representatives of poor and vulnerable workers would foresee this relationship and advocate for their class interests. For Ticchi and Vindigni (2005), at moments of constitutional choice, each income group is capable of making the complex computations required to fully anticipate the translation of the precise income distribution into its interest in the outcome of a coalition game under consensus rules and to contrast that outcome with the majoritarian rule outcome. For Cusack et al. (2007), the economic elites are able to anticipate the costs of being soaked by the poor under PR and contrast this with the long-term economic benefits of maintaining a skilled and happy workforce.

Third, like Marxism, these theories are animated by a sense of functionalist inevitability. For Alesina and Glaeser (2004), if unions are strong and the left is mobilized, PR can be delayed but not avoided. For Ticchi and Vindigni (2005), the socially efficient constitution for a given income distribution will emerge naturally over time. For Cusack et al. (2007), PR evolves naturally as a reflection of the society’s need for economic coordination.

This new class of structural-functionalist analytical history is enormously interesting and constitutes a promising research program in its own right. But there are reasons to hesitate before rejecting the possibility of a causal role for electoral institutions based on analyses that are so lacking in microfoundations and careful historical analysis. A number of questions present themselves. Why did leftist leaders feel that PR was in their interest? Did leftists have a different interpretation of their interests in Britain and other countries that retained SMD, did they face an internal collective action problem, or were they simply too weak to get their way? Is there any evidence in party documents or personal communications of leaders that the relevant actors understood the potential implications of electoral rules for redistribution and expenditures on public goods? How did politicians grapple with situations in which class interests and personal electoral interests may have conflicted? What were the relevant splits within parties, and how did these link up with class and income groups? Is there any evidence that employers’ associations in industries with specific skill investments lobbied the parties of the right to promote PR? How important were the exogenous constraints associated with World War I in strengthening the hand of the left?

A general challenge for empirical analysis associated with sweeping structural macrohistorical arguments is to avoid qualitative or quantitative work that is akin to conveniently selected historical anecdotes. It is helpful to have the claims analyzed by disinterested empirical scholars who are not already wedded to them. Only after the work receives considerable attention do other scholars begin to mobilize dissonant historical information, collect additional data, or conduct robustness checks. Some interesting arguments have been placed on the table about endogenous electoral rules, but more work lies ahead. My sense is that the renaissance in the historical analysis of constitutional choice has just begun. As with the renaissance in the study of democratization in the early 1990s, this will likely be characterized by a productive and critical dialogue between these rational choice structural theories emphasizing the role of class and other social groups as actors and a different class of rational choice theories that feature office-seeking politicians and focus attention on incentives faced by individuals.

The latter perspective has dominated among political scientists seeking to shed light on constitutional choice in new democracies and recent electoral reform in older democracies, where scholars have much better access to information about beliefs and preferences of key actors, internal party debates, and the role of social groups than in interwar Europe (e.g., Benoit 2004, 2006; Moser 2001; Remington and Smith 1996; Kaminski 2002). The clearest lesson in this literature is about the uncertainty of actors over the political and social outcomes that would flow from institutional choices. Virtually every study of Central and Eastern Europe features an episode in which a set of rules advocated by a party or faction had exactly the opposite impact of what was expected, in many cases marginalizing or ending the careers of its advocates.

It seems likely that similar or even greater levels of uncertainty plagued institutional designers in interwar Europe, given the simultaneous doubling or tripling of the electorate and the lack of polling data (Andrews and Jackman 2005). Taagapera (2002) and Shvetsova (2003) go so far as to argue that uncertainty over potential partisan and policy implications of different constitutional alternatives at moments of reform is so great that institutional endogeneity is simply not an issue for scholars interested in examining the impact of electoral institutions on party systems and policy outcomes. In this view, socialists could not possibly have anticipated the long-term implications of PR for coalition dynamics and party development. Perhaps PR could have generated a rancorous split on the left, as in Italy, or partisan fragmentation, as in Weimar Germany.

Endogeneity also becomes less troubling when one thinks about the other central observation in this literature: Political actors seem to have had surprisingly short time horizons, thinking not in broad terms about the strategic interests of the party or its constituents in the long term, but rather about the coalition dynamics of the next election. Andrews and Jackman (2005) point out that during a short-lived pact between the Liberals and Labour in Great Britain in 1906 and 1910, these parties enjoyed a temporary improvement in the

translation of votes into seats, though they had traditionally been disadvantaged by their inferior geographic distributions of support relative to the Conservatives. Of course, the Liberals have suffered in the vote–seat curve ever since, as has Labour to a lesser extent (see Rodden 2006b). PR was very much on the political agenda at the time, but in a series of votes in 1917 and 1918, almost half of Liberal and Labour members of Parliament (MPs) voted against it. By 1924, the Liberals came to understand their mistake and strongly advocated PR. But in spite of the supposed long-term interests of their constituents in PR, the majority of Labour MPs supported the retention of SMD in order to squeeze out Liberal competition for the left-wing vote.

This affair demonstrates the difficulty of structural arguments in which parties are viewed as “carriers” of class interests. A similar lesson can be learned when parties are viewed as aggregations of individuals rather than as unitary actors. Individual leftist incumbents with carefully cultivated bailiwicks or safe seats, for instance, might have incentives to fight reforms like list-based PR. U.S. congressional incumbents often successfully squelch efforts by their own copartisans in the state legislature to improve the party’s position in the reapportionment process because it would do violence to the district boundaries with which they have become comfortable (Butler and Cain 1992).

In short, the recent political science literature makes the move described by Katznelson (1997) in the previous edition of this volume, reacting to ambitious macroanalytic structural work with a style of historical institutionalism that highlights contingent events and uncertain calculations, though largely from a rational choice perspective. In so doing, it challenges the strongest claims of institutional endogeneity implicit in recent structural theories. The verdict is still out, and perhaps it always will be. But at the very least, the strongest claims of institutional endogeneity seem to be unsubstantiated.

I am relatively certain that leftist leaders in early-twentieth-century Europe preferred PR. My intuition is that the reason for this has not yet been emphasized in the literature, though it was recently suggested by Andrews and Jackman (2005): Leftist parties had a relatively skewed geographic distribution of support, owing to the uneven geographic spread of industrialization, and stood to gain in the translation of votes into seats under PR. It may well be true that PR was in many cases the result of leftist mobilization.

Yet, it is not all that difficult to envision that the same countries, with the same background conditions, could have easily maintained SMD and developed smaller welfare states due to some combination of the causal mechanisms discussed previously if only things had gone slightly differently at the crucial constitutional moment. When reading the historical accounts that are not wedded to a theoretical perspective, one is struck by the contingency owing to the specific strategies selected by key actors. For instance, what if the old parties of the right in Sweden and Belgium had more foresight and withstood short-term losses in exchange for long-term gains, like the British Conservatives, by understanding that plural voting and property restrictions were destined for the dustbin of history but holding fast to SMD? Instead, they tried to hold on to

their privileges as long as possible by giving in on PR. At least in Sweden, it is very clear from Verney (1957) that the left would have accepted SMD under the right conditions. It is plausible that this would have locked into place a different set of institutional incentives that could have generated a different tax-transfer regime.

What if Labour in the United Kingdom had paid less attention to vanquishing the Liberals and more attention to competing in the long run with the Conservatives? Or what if the Liberals had faded sooner or merged with Labour? What if World War I had ended differently, and aristocratic elites had regained a firm grip on military and police power in Germany, Austria, and Italy? Perhaps then the constitutional moment would have come and gone without electoral reform in some other countries as well had elites not been so fearful of social disarray.

In historical studies where randomization is not possible, in order to think about causality, we must invoke counterfactuals, and to do so, as Przeworski puts it, “we must be assuming that history could have generated a world different from the one in which we live, that realizations of history other than the actual one are possible” (2007: 9). Given the contingency and uncertainty associated with constitutional decisions, it seems quite plausible that the same countries, with the same social groups and elites, could have developed different electoral rules and welfare regimes.

Natural Experiments

These historical counterfactuals are interesting but frustrating. Like the exclusion restrictions in econometric research using instrumental variables or selection models, the researcher tries to be as persuasive as possible, but belief in causality still requires a rather large leap of faith. This is a distant second-best solution in a world where institutions cannot be randomly assigned to subjects and where it is not possible to observe the same subject under conditions of treatment and control.

But a growing trend in economics and political science is to seek out the rare moments when history bestows an opportunity that resembles randomization. For instance, Banerjee and Iyer (2002) were able to estimate the contemporary impact on productivity and investment of different systems of land taxation set up in colonial India since these systems depended on the date of conquest rather than any characteristics of the districts. The analysis is especially persuasive because the researchers were able to observe a large sample of geographic neighbors that were similar with respect to background conditions but had been subjected to different institutions of land taxation.

The prospects for this type of research in the area of electoral rules may not seem especially good at first glance, but there are a number of possibilities, especially when one examines some of the specific causal mechanisms outlined earlier. Above all, it *is* possible to observe the same population under different electoral regimes. These opportunities are provided by such institutional

features as state and local governments with different electoral regimes than central governments, bicameralism, the contrast between district-level results of presidential and legislative elections, and the difference between national and supranational (European) elections.

The incredible diversity and complexity of electoral institutions in the United States is especially appealing. For instance, in order to test arguments about the importance of the distribution of district-level preferences, it is appealing to contrast the ideological profiles of U.S. Senate delegations from large states, who are elected at large in statewide elections, with those of House members from the same states, who are elected from small SMDs. Local governments in the United States use a wide variety of electoral institutions, including PR. For a long time, the state of Illinois used SMD in one legislative chamber and something akin to list PR in the other.

There are many such opportunities around the world. For instance, the Australian lower chamber uses SMD, while the upper chamber uses PR. Even Britain is becoming more diverse as elections to regional assemblies and the European Parliament are conducted with different electoral rules than national parliamentary elections. Alternatively, one might contrast the behavior of individual legislators elected from districts with those elected from party lists in mixed systems like Germany's.

Second, it may be useful to take a closer look at contemporary instances of electoral reform, like those in Italy, France, Japan, and New Zealand. Of course, these reforms cannot generally be viewed as exogenous, and there are colorful stories explaining electoral reform as Machiavellian moves by brash incumbents. Yet, these moves were often focused on achieving short-term advantage in an upcoming election rather than long-term policy change. The case of New Zealand is especially appealing, since electoral reform emerged essentially by mistake when a Labour MP started a landslide by inadvertently promising a referendum on electoral reform during a televised debate, though the leaders of both parties preferred the status quo (Nagel 2004).

Field Experiments

The move toward experimental research in comparative politics is one of the most striking developments of the past decade. The opportunity to randomly assign institutions to subjects would seem to be rare, but such work is possible for researchers who invest in facilitating partnerships with governments, aid agencies, and development banks. Governments in large countries like Mexico and India have been persuaded to randomize policy treatments or even institutions like reserved local government seats for women and minorities at the level of villages or districts, allowing researchers to examine the causal impacts of the policies or institutions in a way that requires far fewer assumptions than observational research. Chattopadhyay and Duflo (2004) examine the effects of female representation by exploiting the random reservation of village council head positions for women in West Bengal and Rajasthan. Olken (2007)

randomly assigns institutional mechanisms used for communities to select infrastructure projects in Indonesia, and Humphreys and Weinstein (2007) generate a scorecard rating the performance of members of the Ugandan parliament, and randomize its dissemination across geographic constituencies in order to assess the impact of information and transparency on the performance of elected officials. Most relevant to the discussion in this chapter, Beath, Enikolopov, and Christia (in progress) are able to randomly assign some democratically elected local government councils in Afghanistan with majoritarian election procedures, and the others with “cluster-based elections,” while the rest remain without such councils. This type of research will undoubtedly expand tremendously in the years ahead.

CONCLUSIONS

By focusing on electoral rules, this chapter has identified a pattern that characterizes institutional research more generally. Over several decades, researchers have established some interesting conditional correlations using cross-country data. In addition to the relationship explored in this chapter, some highlights include electoral rules and the number of parties, federalism and welfare expenditures, and democracy and economic development. These correlations fueled the creativity of theorists, who developed competing accounts of the underlying causal mechanism. In many cases, these accounts led to a wider array of testable empirical propositions. Along the way, questions arose about the direction of causality and the possibility of omitted variables, casting doubt on the causal role of institutions in explaining outcomes. Straightforward applications of the rational choice institutionalist perspective morphed into debates about whether institutions matter at all.

To get to the bottom of things, while a smaller strand of literature seeks out natural experiments and, more recently, field experiments, two related empirical research strategies have dominated thus far: instrumental variables and analytical history. In fact, I have argued that the successful implementation of the former often requires the latter. In both cases, researchers are looking for truly exogenous variables to help identify causality in a two-stage model where institutions are selected in the first stage and policy or macroeconomic outcomes are explained in the second stage. As a result, we have witnessed a rapid return to fashion of the sweeping structural-historical analysis praised by Katznelson (1997) in the first edition of this volume, although with a rational choice starting point. As scholars attempt to sort out the possible causal role for institutions, these structural analyses contrast with rational choice theories that focus more squarely on the decisions of self-interested individuals at crucial moments of constitutional choice.⁶

⁶ Though it is often mentioned in passing, analysis of culture as a determinant of electoral institutions has lagged behind, perhaps above all because of the difficulty of measurement, especially in settings where institutional choices were made in the distant past.

Consider one aspect of the literature on federalism. Like SMDs, a federal constitution appears to be correlated with lower welfare expenditures (for a literature review, see Castles, Obinger, and Leibfried, 2005). Researchers have come up with a number of possible causal mechanisms, including status quo bias owing to multiple veto players during the era of welfare state expansion, interjurisdictional tax competition, sorting by the wealthy into their own jurisdictions, powerful and conservative courts, and the overrepresentation of conservative, sparsely populated regions in upper legislative chambers. These arguments have led to interesting empirical studies, but each mechanism generates questions about institutional endogeneity. Perhaps fragmented authority, tax competition, and malapportionment reflect successful schemes by wealthy elites to limit the political power of the poor and undermine redistribution. The current forefront in this literature features theories that make the varieties of federalism endogenous, and empirical analyses that include instrumental variables and analytical history (for a review, see Rodden 2007). Again, there are structural theories in which institutions are driven by factors like the interregional and interpersonal distribution of income (e.g., Bolton and Roland 1997; Boix 2003), contrasted with theories focusing on office-seeking politicians (e.g., O'Neill 2003), while some of the work weaves these perspectives together (e.g., Diaz-Cayeros 2006).

A similar pattern characterizes the much larger literatures on democracy and economic development. The correlation has been in the literature for a long time, but the 1990s saw a renaissance in thinking about the direction and nature of causality, accompanied by a great deal of innovation in both theoretical and empirical analyses, spurred largely by the contributions of Adam Przeworski and his collaborators. Puzzles about the endogeneity of democratic institutions inspired the award-winning work of Boix (2003) and Acemoglu and Robinson (2006). Again, there seems to be a productive friction between these neostructural-historical works featuring groups like “the poor” and “the elites” as actors and works focusing on self-interested politicians (e.g., O'Donnell and Schmitter 1986; Przeworski and Limongi 1997).

In conclusion, I return to the questions posed in the introduction. Is the newfound concern with causality adopted from economics turning attention away from the thorny puzzles and big questions that make the field of comparative politics interesting? On the contrary, by turning to historical data in search of natural experiments and instruments, researchers have breathed new life into some of the classic questions of the subfield. In fact, it was a handful of economists puzzling over the endogeneity of political institutions who returned to the sweeping structural approach to analytical history favored by scholars like Barrington Moore, Reinhard Bendix, Gregory Luebbert, and Dietrich Rueschmeyer, now dressed up with game theory.

It is tempting to add Karl Marx to the list. Due to the influence of methodological individualism, theories featuring unitary actors like “the elites” and “the poor” were as unfashionable as Marxism itself in the very recent past. Out were strong assumptions featuring farsighted, well-informed individuals whose preferences were driven by their place in the income spectrum or social class

structure, who could effortlessly overcome collective action problems and translate individual into collective rationality. In were self-interested politicians and interest group leaders whose behavior might or might not link up with the interests of their constituents. It was this rationalist literature focusing on individual politicians, in fact, that generated many of the causal claims about institutions based on the notion that they provided constraints shaping the incentives of office-seeking politicians.

It is not surprising, then, that in the neostructural literature, where collective action problems and historical contingency play no role and class groups rather than individuals are the movers of history, institutions begin to feel like manifestations of “deeper” forces, as in Marxism. For political scientists steeped in the individual rational choice framework, institutional choice appears to be more contingent, often driven by fleeting coalition dynamics or quixotic efforts to stem vote loss or undermine a rival in the next election by a troubled incumbent with limited information. In this view, the conditions that underpin institutional choice seem unlikely to be correlated with political or economic outcomes 100 years later, and causal claims about institutions seem less problematic.

These literatures generate a creative tension that is driving the institutional literature toward new ways of understanding the emergence and stabilization of institutions. Both perspectives can provide correctives to the excesses of the other. Stories focusing only on highly contingent strategic interactions in specific countries are almost certainly missing some of the “patterns that are hidden and deep” (Lichbach this volume). Institutions reflect the interests of powerful groups in society at the time they were adopted, and institutional reform is likely constrained thereafter by those same groups, as well as by a different constellation of interests generated anew by the institutions – for example, incumbents, judges, and civil servants – as well as other groups that emerge decades after institutions have been adopted. Yet, at this point, even the best structural-historical theories are extremely simplistic, one-shot stories that are badly in need of microfoundations. Theoretical and empirical work on endogenous institutions will continue to be at the top of the research agenda in comparative politics for some time.

Finally, are comparative institutional researchers making any progress in solving identification problems? Perhaps the most believable studies are those that unearth natural experiments or are able to craft randomized field experiments. But for the lion’s share of observational data on which the field relies, there are no econometric quick fixes. Like qualitative analytical historical work, all involve fairly strong assumptions and assertions about what is ultimately exogenous. Instead of viewing history as a tangled bundle of “unobservables,” the way forward is to delve into theory-guided exploration of the past and grapple with complex questions for which completely satisfying answers will always be elusive.

It is easy to criticize the work of almost any empirical researcher by spinning a story about endogeneity. What seems like a trivial critique to one scholar appears devastating to another, and consensus about these matters is nowhere in sight. Given that natural and field experiments can seem rare, expensive, and unsuitable to the research questions of so many scholars, it is tempting to give up

on the whole enterprise and go back to the comfort of correlations and the excitement of big questions, choosing not to worry about the potential biases contaminating inferences.

But advocates of such an approach should be aware that the opposite reaction to the problems of causation and endogeneity is gaining strength – what might be called the “extreme experimentalist position.” In a provocative article, Gerber, Green, and Kaplan (2004) imply that observational studies should be accorded zero weight (and given zero funding) if they cannot produce believable information about the nature and extent of the bias plaguing their inferences. The idea is that it is better to have a few unbiased, “secure empirical premises” from which to build, even if these are not particularly interesting, than to “grasp for weak tests of expansive and arresting propositions” (25).

An extreme interpretation of this argument is that observational research should simply be abandoned. One might even go on to argue, as statisticians sometimes do, that elaborate theories about causal mechanisms, counterfactuals, and structural models are a waste of time and resources, and that political science should only concern itself with a “barefoot empiricist” notion of causality, establishing unadorned facts in the laboratory or through field experiments. The thicket of problems like unobserved heterogeneity, selection, errors in variables, and data mining are simply too daunting to attack with theory and observational research. The solution is to throw up our hands and rely exclusively on randomization.

The most extreme experimentalist position is not likely to win the day in comparative politics. After all, absent some theories and a base of observational research, it is difficult to know what experiments to run or with what kinds of subjects to run them. For some research questions, the threats to external validity of experimental results will simply be too great. It is not clear how much we might learn about the impact of electoral rules on the welfare state by randomly assigning proportional electoral rules to small villages in postconflict situations where international agencies have the necessary clout. Some of the causal mechanisms laid out earlier are driven by a kind of geographic heterogeneity that is not possible within a village. Moreover, if institutions are equilibria that must be self-enforcing to survive, what we learn by temporarily imposing them in a contrived way might be illusory. Experiments in comparative politics are likely to emerge as valuable complements rather than substitutes for observational studies.

Yet, comparative politics ignores the experimentalist critique at its peril. If the field has nothing to offer but weak tests of expansive propositions, it will quickly become quaint and obsolete. Observational data can be used in conjunction with experimental research in efforts to build the firmest possible base of knowledge, but this requires that observational researchers be as clear as possible about the biases of their claims and innovate in their use of theory and empirical technique to reduce them. Progress in sorting out causal claims through empirical research is frustrating and slow, and for most institutional researchers, the demonstration of causality will remain a Platonic ideal that cannot be achieved in practice. But imperfect efforts to approach it are far better than the alternatives.

The Comparative Political Economy of the Welfare State

Isabela Mares

Comparative research on the welfare state goes back to the mid-nineteenth century. As such, it is as old as the welfare state itself. Fabians looked for arguments on how to help the poor and also minimize distortions from state interventions. In Germany, *Kathedersozialisten* such as Gustav Schmoller and Adolph Wagner searched for policy instruments that could lift citizens out of poverty while cementing their allegiance to the prevailing political order (Schmoller 1900; Wagner 1892; cf. Backhaus 1997).

Over the past three decades, the literature exploring the causes and distributive implications of various social programs has grown at a dramatic pace. Methodologically, the study of social policy has provided fertile ground for the interplay of structuralist, rationalist, and culturalist perspectives. Structuralist scholars have explored how processes such as state-building (Rokkan 1974) or industrialization (Rimlinger 1971; Wilensky 1975) impacted differences in spending. Culturalist explanations have drawn attention to cognitive biases of policymakers in accounting for changes in policy (Weyland 2004, 2007) or stressed the importance of societal norms and values, such as altruism, in conditioning public support for redistribution. Rationalist scholars have contributed to our understanding of the social policy preferences of societal actors – such as unions or employers – and of politicians who balance office-seeking and ideological commitments. They have also drawn attention to the *strategic* component of the process of bargaining over various social policies and shown how differences in institutional and political rules constrain this strategic interaction (Kitschelt 2001; Mares 2000, 2001b; Iversen and Soskice 2006).

Critical theoretical and empirical advances in the field have not come from a head-on clash among these theoretical paradigms. The major studies that have reoriented comparative research on the welfare state – such as Gøsta Esping-Andersen's *Three Worlds of Welfare Capitalism* – have proposed explanations that blend elements from all three perspectives (Esping-Andersen 1990). While recognizing that underlying structural conditions – such as the level and speed of industrialization – alter the balance of power among political actors representing different constituencies, this study attributes differences in

spending to differences in political partisanship. It also illustrates how the social policy demands of Social Democratic, Christian Democratic, or liberal parties are conditioned by the interests of their central political constituencies but also by normative beliefs about the appropriate role of the state, the family, and the market in distributing social transfers and services (Esping-Andersen 1990; Van Kersbergen 1995; Huber and Stephens 2001). Other seminal studies – such as Theda Skocpol's *Protecting Soldiers and Mothers* or Paul Pierson's *Dismantling the Welfare State* – have attributed an important causal role to political institutions in shaping variation in the development of different social programs and in their capacity to resist pressures for retrenchment (Skocpol 1992; Pierson 1994). Institutions, these studies argued, have both material and ideational consequences. They affect the material resources held by different groups, reinforcing or changing their relative distribution of power but also causal beliefs – about the fairness or effectiveness of particular policy interventions – held by politicians or mass publics.

Recent scholarship has made two significant contributions to this subfield of comparative politics. First, this research has formulated much crisper theories of individual-level variation in the social policy preferences of wage-earners or employers, showing how structural characteristics of an economy, such as the level of inequality, the distribution of skills and risks affect individual preferences for insurance and redistribution (Iversen and Soskice 2001; Moene and Wallerstein 2001; Swenson 2002; Mares 2001). Secondly, these explanations have demonstrated that the process of bargaining over different social policy outcomes involves strategic accommodation and compromise. Political actors – such as labour unions, employers' associations or parties – rarely succeed in adopting their preferred social policy outcome and end up compromising around policies that are “second-best” alternatives (Mares 2001b; Hacker and Pierson 2002). In these accounts, underlying structural and institutional variables affect not just the resulting policy outcome, but also the bargaining process. They affect the range of alternatives over which actors bargain and the set of ‘strategic alliances’ and political coalitions that form during this process (Mares 2001b, 2003).

While methodologically eclectic, the literature on comparative social policy has been empirically quite parochial. Nearly all theories accounting for the development or reform of major programs have focused on the policy experience of advanced industrialized economies, a narrow universe of cases comprising somewhere between fourteen to eighteen countries. The high multi-collinearity among explanatory variables has severely hampered the ability of scholars working in this sub-field of comparative politics to test various theoretical explanations against each other. In the face of this constraint, many studies have chosen to assert, rather than prove, the triumph of one explanation over its alternatives. In recent years, a number of studies have broadened the scope of empirical analysis and begun to explore the determinants and economic consequences of social programs in Latin America, Eastern Europe, and Asia (Castiglioni 2005; Brooks forthcoming; Haggard and Kaufman forthcoming;

Kaufman and Segura Ubiergo 2001; Madrid 2003; Weyland 2004, 2007; Wong 2003; Heckman and Pagés 2004; Huber, Pribble, Nielsen and Stephens 2006).

This chapter surveys the main traditions of research examining the determinants of social programs enacted to ameliorate poverty or provide insurance in moments of unexpected drops in income. The chapter has two objectives. One is to explore how different studies have reconciled the relative importance of structural, institutional, and ideational variables in accounting for the evolution and change in social policy. The other is to evaluate whether existing explanations developed in the context of advanced industrialized economies are robust and can account for the variation in the structure of social programs and levels of social spending in a much larger universe of cases. I seek to examine how well existing theories can account for “out-of-sample” cases. Given the broad scope of the literature on social protection, my review will of necessity be highly selective, prioritizing themes taken up by studies of developed and developing countries alike.

THE STRUCTURAL DETERMINANTS OF SOCIAL SPENDING: THE ROLE OF ECONOMIC DEVELOPMENT

The earliest research on the welfare state sought to identify the broad structural preconditions of spending. This line of inquiry goes back a long time historically. In 1892, Adolph Wagner conjectured that higher levels of income would be associated with higher levels of spending on social programs (Wagner 1892). Several decades later, economists reformulated Wagner’s hypothesis, suggesting that income elasticity for publicly provided goods and services was positive. As such, a rise in the income of a representative household was expected to bring about a rising demand for these goods and services, which, in turn, would result in higher social spending. Empirical studies exploring the impact of economic development on the level of spending among advanced industrialized countries have reported mixed results. Aaron (1967) and Wilensky (1981), and more recently Franzese (2002), found a positive correlation between economic development (measured as per capita gross domestic product [GDP]) and social spending during the post–World War II period. In a recent study of the historical origin of social programs in advanced industrialized countries between 1880 and 1930, Lindert found that economic development had a statistically significant effect on social spending, but that the substantive magnitude of this effect was quite weak (Lindert 2004b: 52). Other studies found no effect of level of development or economic growth on social spending in advanced industrialized countries (Hewitt 1977; Castles 2000).

Studies examining the impact of economic development on social spending in a broader universe of cases that includes both developed and developing countries have also yielded ambiguous results. In an early study, Cutright (1965) examined the correlation between economic development (measured as energy consumption) and the length of experience with various social programs and found a positive association between these variables in a sample of 76 countries. Wilensky (1975)

also reported that economic development is positively related to social security spending in a sample of 60 countries. Other studies, however, suggested that the effect of economic development disappears once additional economic and political variables are controlled for (Pryor 1965; Zschock 1983; Adsera and Boix 2002). In a broad universe of cases, the level of economic development cannot account for differences in policy design, such as the scope of social protection and the socialization of risks (Mares 2005).

For other scholars, the primary causal factor accounting for the development of social programs was not economic development per se but the labor market transformations triggered by the process of industrialization. This line of research goes back to one of the foundational books of political economy, Karl Polanyi's *The Great Transformation* (Polanyi 1944 [1957]). Polanyi observed that the constitution of relatively unencumbered labor markets was followed by a "countermovement" in which "society protected itself against the perils inherited in a self-regulating market system" (76). This sweeping hypothesis did not spell out the exact causal mechanisms by which industrialization led to the establishment of policies of social protection. Nor did it attempt to provide an account of the temporal and cross-national variation in the design of policies and programs. In fact, Polanyi was equally intrigued by the *similarities* in the political responses adopted by European liberal and conservative leaders during the nineteenth century. "Victorian England and the Prussia of Bismarck were poles apart and both were very much unlike the France of the Third Republic or the empire of the Hapsburgs. Yet each of them passed through a period of free trade and laissez-faire, followed by a period of antiliberal legislation in regard to public health, factory conditions, municipal trading, social insurance, shipping subsidies, public utilities trade associations, and so on. It would be easy to produce a regular calendar setting out the years in which *analogous* changes occurred in the various years" (147).

The main form of social spending during the early period of industrialization was poor relief. Aggregate social policy expenditures never exceeded 2 percent of GDP, a tiny figure when compared to the recent scope of spending. In a recent study evaluating the determinants of social spending during the period 1750–1880, Lindert finds that electoral institutions mediated the consequences of industrialization on redistributive spending. In earlier periods, when suffrage was restricted to landowning elites, spending on the poor was relatively generous, as these actors tried to forestall the movement of peasants to the cities. The extension of suffrage to groups such as "professionals, shopkeepers and yeoman farmers" brought about a reduction in spending on the poor (Lindert 2004a: 80). The newly enfranchised groups feared higher taxes and were reluctant to support persons who might choose not to work. It was only the advent of full suffrage that brought about an increase in spending. Lindert's empirical analysis thus qualifies Polanyi's conjecture, suggesting that the countermovement to the process of industrialization was more likely to occur under either highly restrictive or full suffrage, but not in the intermediate cases.

Studies of the determinants of social spending in OECD countries during the postwar period report positive associations between the size of the labor force in

industry and social spending (Stephens 1979; Korpi 1978, 1983), while others suggest that the declining rates of employment in agriculture and industry are more accurate predictors of the growth in spending (Iversen 2001). Outside OECD economies, some authors have noted the existence of strong cross-regional differences in levels of industrial employment between East Asian and Latin American economies and have attempted to link these developments to differences in social spending (Birdsall, Ross, and Sabot 1995; Fields 1994; de Ferranti, Perry, Indermitt, and Serven 2000; Kaufman and Haggard forthcoming). Even after controlling for underlying differences in the growth rate, manufacturing employment and the level of real wages grew at a much higher rate in East Asia compared to Latin America. The implied mechanism linking these diverging labor market trajectories to levels of spending is different from the Polanyian logic. In contrast to workers in precarious conditions of self-employment, industrial workers have a higher ability to finance larger programs. Economies with higher levels of manufacturing employment have higher fiscal capabilities to finance larger welfare states, rather than higher levels of demand for social spending.

OPENNESS

Another line of research has examined the social policy consequences of larger aggregate trade flows or of volatility in the terms of trade on social spending. This research builds on pioneering work by David Cameron that identified a positive relationship between trade flows and the size of the public sector in a sample of 18 OECD economies during the period 1960–1975 (Cameron 1978). To account for this empirical regularity, Cameron hypothesized that governments enact income supplements or social insurance schemes to compensate workers whose income and employment are threatened by external competition. Other scholars have supplemented Cameron’s initial statistical results with qualitative research on the industrial and social policies pursued by small countries with encompassing interest associations, documenting how these economies “complemented their pursuit of liberalism in the open economy with a strategy of domestic compensation” (Katzenstein 1985: 47; Ruggie 1982). In recent years, a number of studies have updated these results for the period through the mid-1990s (Garrett 1998; Garrett and Mitchell 2001) or for the other great era of economic globalization, the period between 1850 and the onset of World War I (Huberman and Lewchuk 2003). Related studies have explored the consequences of volatility in the terms of trade for social spending, demonstrating a positive correlation between these two variables in a sample of over 100 countries (Rodrik 1997, 1998).

While Cameron’s and Rodrik’s studies share many commonalities, they postulate, in fact, two distinct mechanisms linking economic openness and larger public sectors. In his 1978 article, Cameron hypothesized that openness contributes to higher levels of industrial concentration (Cameron 1978: 1256). This, in turn, leads to high levels of unionization and an increase in the political strength of left-wing parties that enact encompassing social programs. Rodrik suggested a different causal pathway, one working through the economic

insecurity experienced by the representative household (Rodrik 1997, 1998). As Rodrik elaborates, “more open economies have greater exposure to risk emanating from turbulence in world markets. One can view larger government spending in such economies as performing an insulating function, insofar as the government sector is the ‘safe’ sector (in terms of employment and purchases from the rest of the economy) relative to other activities, and especially compared to tradables” (Rodrik 1998: 1011). In this account, the main function of social policy is to reduce the volatility in income of households in the tradable sectors.

In either specification, economic openness affects social spending through a very long causal process. However, most empirical studies gloss rather quickly over many intervening variables and only test the relationship between the two endpoints in this long causal chain. This remains an important limitation of this line of research. More studies exploring the intermediate steps of this causal relationship are needed. One important avenue of future research is to develop further empirical tests of the micro-logic underpinning this relationship by exploring how external insecurity affects individual social policy preferences. At this point, we don’t know what the impact of externally induced economic insecurity on individual-level support for social spending is and no study has tested for the causal impact of this variable on preferences for social spending using individual-level survey data. Additional research tracing out the political process by which external insecurity leads to changes in social policies is also needed. One shortcoming of existing studies is that they focus exclusively on the demands of workers facing high levels of economic insecurity, underestimating the political opposition to larger social programs or more encompassing social policies. If sectors facing low volatility in income are politically pivotal, then an increase in openness will not be associated with an expansion of social protection (Mares 2004). Finally, it is important to explore how preexisting policies and institutions mediate demands for social protection resulting from higher levels of insecurity. If states have a low extractive capacity and are unable to provide sufficient guarantees for future streams of social policy transfers, then it is harder to construct an encompassing coalition supporting higher levels of spending. Thus, the fiscal weakness of states affects the strategies of groups facing high levels of economic insecurity. In recent research, I have pursued this argument, showing that the effects of economic insecurity on spending are contingent on the administrative capacity of states (Mares 2005). These results challenge the view of a linear relationship between external insecurity and social spending, suggesting that the impact of openness is contingent on additional political variables (see also Adsera and Boix 2002).

The Systematic Clustering of Policies: Accounting for Cross-National Differences among Welfare State Regimes

Both lines of research have focused only on very broad levels of social spending and have downplayed differences in policy design across countries. But the

salient political decisions that bring about a new program or that sharply transform the scope of its application are never really decisions about aggregate spending levels. More commonly, they concern the scope of coverage, the conditions of eligibility for benefits, and the relative mix of private and public institutions in the delivery of social transfers. Critics of these perspectives on social policy correctly pointed out that “expenditures are *epiphenomenal* to the theoretical structure of the welfare state” (Esping-Andersen 1990: 20; emphasis added). This forceful critique has reoriented theoretical and empirical research on the welfare state from a study of differences in the level of spending to a study of differences in the design of policies.

The foundational study that marked this reorientation in the study of welfare states was Gosta Esping-Andersen’s *Three Worlds of Welfare Capitalism* (Esping-Andersen 1990). This study is premised on the assumption that two latent dimensions capture empirical variation among welfare states. The first is “decommodification,” the implicit guarantee to individuals to “maintain a livelihood without reliance on the market” (Esping-Andersen 1990: 22). Social policies that take high values along this dimension provide high replacement rates, allowing citizens “to opt out of work whenever they consider it necessary” (23). The second dimension is “stratification.” While many social policies attempt to counteract and minimize the inequality generated by the labor market, social policies themselves remain a source of inequality. Stringent eligibility rules can stigmatize the recipients of benefits; occupational benefits can reinforce and sometimes even magnify the inequalities created by the labor market. Hence, welfare states “re-stratify” (Esping-Andersen 1990: 24).

Building on earlier efforts to classify systems of social protection (Titmuss [1958] 1974), Esping-Andersen documents that welfare states in advanced industrialized countries form three distinct clusters. Liberal welfare states attempt to minimize interference with the market, prioritize self-help, and rely extensively on privately provided benefits. Conservative welfare states are heavily based on insurance schemes, which reinforce occupational differences. By contrast, social democratic welfare states rely extensively on tax-financed, citizenship-based benefits. The primary evidence for the clustering of welfare states in three distinct regimes comes from a cross-sectional analysis of differences in program design among 14 OECD countries in 1980 (Esping-Andersen 1990). Subsequent studies provided more rigorous tests of the hypothesis of clustering of OECD countries in three distinct welfare regimes (Obinger and Wagschal 1998) and updated these initial measures by developing time-varying indicators of decommodification and stratification (Scruggs and Allan 2006).

To explain the historical emergence of distinct welfare regimes, Esping-Andersen invokes the causal interplay among a wide range of variables (Esping-Andersen 1990). In certain continental European countries, such as Germany and Austria, conservative leaders established insurance programs during the last decades of the nineteenth century in an effort to stem the political rise of working-class parties. By contrast, in Scandinavian countries, Social Democrats in coalition with agrarian parties supported the introduction of tax-financed,

universalistic programs (Esping-Andersen and Korpi 1984, 1985). In countries where either left-wing power or conservative, nondemocratic forces were weak, welfare states expanded through an extension of existing policies and practices of poor relief. While this account is able to identify distinct constellations of political forces supporting policies that differ in their mode of financing and extent of coverage, it is less successful in accounting for the persistence of political support for these policies over long historical stretches in the face of massive economic and political transformations.

Esping-Andersen's study has reoriented dramatically research on the welfare state. Numerous studies have reconfirmed the results of that study, proposing small amendments to its main theoretical propositions (Birchfield and Crepaz 1998; Scharpf and Schmidt 2000; Huber and Stephens 2001). The bold hypotheses advanced by this study have generated numerous debates, many of which are still ongoing. One such controversy concerns the number of welfare regimes and the attribution of specific countries to particular regimes (Castles and Mitchell 1993; Ragin 1994a; Ferrera 1996). Other scholars have argued that this typology focuses exclusively on social security and income transfers and omits important programs, such as health and welfare services (Leibfried and Mau 2007), education policies (Allmendinger and Leibfried 2003), or policies that shape the incentives of women to allocate time among paid work and caring responsibilities (Orloff 1993).

The political explanation advanced by Esping-Andersen has come under attack from two different perspectives. One objection, forcefully articulated by Peter Baldwin, contests the assumption that parties representing working classes have been the main historical force accounting for the introduction of universalistic policies. As Baldwin argues, "although it may have suffered more than its share of risks, the proletariat had had no monopoly on uncertainty or an interest in measures to ameliorate such circumstances. Nor has the industrial working class been *the* risk-prone group in every country at the time social insurance was first developed" (Baldwin 1990: 12; emphasis added). In Baldwin's alternative account, the middle classes are the pivotal political group that influences the choice among different programs. Universalistic social policies were introduced in Scandinavian countries at the turn of the twentieth century by parties representing smallholders and other independents. Thus, "the view of an essential link between the apparent solidarity of early Scandinavian welfare policy and the Social Democrats is misleading. It anachronistically reads back a misunderstanding of . . . [post-World War II] reforms to an earlier period when other factors were at work" (Baldwin 1990: 62).

Other studies have challenged this approach for overstating the conflict between capital and labor. Esping-Andersen's study, premised on the notion of decommodification, already *presupposes* a zero-sum political conflict between these actors. Since decommodification "strengthens the worker and weakens the absolute power of employers . . . employers have always opposed decommodification" (Esping-Andersen 1990: 22). This assumption is too strong and, in fact, empirically inaccurate. New studies covering cases as diverse as

Bismarckian Germany, the French Third Republic, Social Democratic Sweden, and the United States during the New Deal (Gordon 1994, 2003; Jacoby 1997; Mares 2001b, 2003; Swenson 2002) have documented widespread heterogeneity in the preferences of employers and in the political strategies they pursued to achieve their preferred social policy outcomes. The common finding of these studies is that major social insurance programs were not introduced against the will of employers, but were supported by key sectors of the business community.

The exploration of the sources of business interest in social insurance poses intriguing puzzles. Under what conditions do self-interested, profit-maximizing firms support policies that impose some costs on them and provide benefits to their employees? If employers stand to gain from social policies, what are the specific advantages provided by such policies? What factors affect the variation in the social policy preferences of firms and the mix between support for and opposition to a new program? One line of scholarship suggests that broad institutional variables of the political economy shape the social policy preferences of employers (Kitschelt, Lange, Marks and Stephens 1999; Hall and Soskice 2001; Manow and Ebbinghaus 2001). “Varieties of capitalism” scholars distinguish between “coordinated” and “liberal” market economies. In coordinated market economies, a dense network of business organizations, as well as the availability of patient capital, facilitates high levels of investment in firm- or industry-specific skills. By contrast, in liberal market economies, firms’ incentives to make long-term investments in the skills of their workers are lower due to weak employers’ associations and fluid capital markets. In coordinated market economies, the joint investment in skills by workers and employers cements a strong alignment of interests among these actors. In these economies, employers support a wide range of labor-friendly policies and institutions, which include the regulation of labor markets, the coordination of wage bargaining, and the provision of social benefits. By contrast, in liberal market economies, we are more likely to encounter antagonism between capital and labor. Here, employers will oppose policy initiatives that regulate labor markets and provide social benefits (Wood 2001).

Yet, employers are not monolithic actors, either in coordinated or liberal market economies. One limitation of this perspective is that it overemphasizes coherence in the interests of capitalists. It does not specify additional variables that can account for variation in the social policy preferences of firms within each type of economy. The empirical evidence accumulated over the past few years provides only partial support for the hypotheses advanced by varieties of capitalism scholars. While identifying differences between coordinated and liberal market economies, most studies have documented much wider heterogeneity in the social policy preferences of firms that cannot be attributed to overarching institutional variables. Other factors, such as the level of competition in product markets (Swenson 2002), the existence of private policies (Hacker 2002), and the political contacts established by the firm, matter too (Martin 1990).

In *The Politics of Social Risk*, I seek to identify the factors that affect variation in the social policy preferences of firms and also specify the conditions under which cross-class alliances form (Mares 2003). This book shares the premise of varieties of capitalism studies that social policies protect the investment made by firms in workers' skills. Social policies with earnings-related benefits raise the "reservation wage" of higher-skilled workers and do not require them to accept jobs that are not commensurate with their qualifications (Mares 2001a). As such, both employers and employees who have made irreversible investments in skills will favor contributory insurance policies. But other variables in addition to skill level also matter in shaping social policy preferences and open distinct cleavages among workers and employers. One such variable is the distribution of labor market risks across occupations. Since social policies mandating contributions that are not tied to the incidence of a risk redistribute across risk categories, we expect high- and low-risk industries to have divergent policy preferences.

The recognition that employers have been active participants in the creation of the welfare states does not imply that organizations representing the interests of workers have been passive bystanders. Nor does it imply that employers have been agenda setters in welfare state reform. The key finding of these studies is that major, enduring social policy legislation has been supported by cross-class alliances among capitalists and workers (Swenson 2002). If a large number of sectors and workers face a high incidence of risks, the dominant cross-class alliance will support social policies with broad levels of coverage. By contrast, if only a narrow segment of the workforce faces high levels of risks, the dominant coalition will support "private-type" policies that link insurance charges to the incidence of a risk. An increase in the level of investment in skills increases the support of both employers and employees for social insurance programs with earnings-related benefits.

While significant progress has been made in the exploration of business preferences, our understanding of the pathways by which employers influence the adoption of new social policies remains imprecise. Several scholars recognize that the process of bargaining over new social policies has a strategic component, and that both workers and employers might support policies that are not their preferred outcome (Hacker and Pierson 2002; Mares 2001b, 2003; Swenson 2002). Many (but not all) cross-class alliances can be "strategic alliances" based on the second-best preference of these actors. While this is theoretically plausible, it is often difficult to distinguish empirically among underlying preferences and strategic motives. The difficulties are often exacerbated by the absence of private archives that allow us to study the preferences of these actors prior to the bargaining process (Mares 2003). Finally, these studies provide very little systematic theorizing about the impact of additional political variables (such as partisanship) on the bargaining process among actors with different policy objectives and about the probability of formation of different cross-class alliances. These limitations of current scholarship open up ample research opportunities for future studies.

Welfare Regimes in Developing Countries

In recent years, scholars of social policies in Latin America, Asia, and Eastern Europe have sought to identify and map out variation in welfare regimes in these regions. Empirically, these studies have focused on differences in aggregate levels of spending and differences in spending categories across programs. This is a highly promising line of research that is still characterized by an intense search for the “first-order” causal variables that might explain the initial adoption of a set of policies and by efforts to establish firm theoretical and empirical links between these variables and the preferences of key political actors.

One set of explanations seek to identify the underlying structural factors that condition the choice of a developmental model and a social policy model (Wibbels and Ahlquist 2007). As these studies argue, the latter two variables are causally linked. A particular developmental model – premised on either “import-substitution industrialization” (ISI) or export expansion – requires a steady supply of labor. Social policies play a critical role in affecting workers’ decisions to participate in the labor market (and to choose work over subsistence) and the types of education and skills they acquire. Wibbels and Ahlquist argue that the resulting social policies tend to cluster on three main regimes that emphasize either redistributive social insurance, human capital development, or a mix of the two.

This study suggests that four structural conditions – the domestic market size, relative abundance or scarcity of labor, asset inequality, and the openness of the international economy – set in motion distinct developmental strategies. In countries marked by large domestic markets, scarce labor, and high inequality, inward-oriented ISI development strategies emerged in response to the Great Depression. Capitalists sought to create a labor force capable of domestic industrial production, as well as a consumer base capable of absorbing its output. To do so, they needed to invest in the education of their workers, and they looked to social policy as a way to retain workers by insuring them against the risks of injury or sickness. Urban industrial workers came to possess a high level of bargaining power in these labor-scarce economies, and were able to organize effectively to lobby for what Wibbels and Ahlquist call “insurance-based social regimes.” Capitalists supported this redistribution through social security programs because it ensured continued demand for their domestically produced goods.

In contrast, in large, labor-abundant, and equal economies, ISI policies were staved off through the efforts of relatively stronger rural sectors that were disadvantaged by the urban bias of ISI. In these cases, an option for “export-led industrialization” (ELI) was created. Producers required a workforce with a general education, but only if labor costs were kept low to maintain their ability to compete in international markets. The social policy mix favored by these employers emphasized tax-financed education and health care but relatively low spending on insurance programs. In these economies, the relative abundance of labor prevented the emergence of collective action among workers in support of increased redistribution. Wibbels and Ahlquist refer to this as a “human capital” spending regime.

Finally, Wibbels and Ahlquist note a third “mixed” social spending regime that combines “modest levels of insurance and human capital spending.” This occurred in countries with large domestic markets and abundant labor, but where ISI developed due to high levels of inequality. The abundance of labor made it impossible for workers to collectively demand high insurance spending, leading to only moderate redistribution and the adoption of some human capital spending. Wibbels and Ahlquist also suggest that such mixed outcomes may occur in smaller countries with mineral wealth.

This elegant and parsimonious theory presents a novel explanation for the determinants of social spending in developing countries. However, several aspects of this explanation require further analysis. First, the relationship between the underlying structural variables and developmental models needs further clarification. Many of the structural factors taken by Wibbels and Ahlquist as given and exogenous might in fact be endogenous to the political process and the social policy regime adopted. In particular, levels of inequality and labor market stratification would seem to be as much created by the level of redistribution or the education system as they are products of it. Labor scarcity, one of the key explanatory variables in the Wibbels–Ahlquist model, is also treated as “country-invariant,” whereas, in fact, countries show significant variation over time in this variable. As the model relies on time-invariant structural variables, it cannot account for temporal changes in economic policies pursued by different countries. Notable discontinuities – such as Korea’s shift from an ISI to an ELI model in the mid-1950s – cannot be accounted for in this framework (Chibber 2003). Second, in specifying the relationship between developmental models and social spending, Wibbels and Ahlquist assume that all leaders facing similar structural conditions will happen upon development strategies with similar implications for social policy spending. As such, this study overlooks the importance of consideration of political survival made by leaders in both developed and developing countries (Mares and Carnes forthcoming; cf. Haber 2007).

Haggard and Kaufman (forthcoming) draw on similar structural variables to explain the determinants of developmental regimes and the choice of social policy, but they differ from Wibbels and Ahlquist in emphasizing the importance of “critical alignments” that occurred in the mid-twentieth century in different regions of the world. They note significant differences in spending levels in three areas – health care, pensions, and education – as well as differences in aggregate levels of spending among welfare regimes in Latin America, East Asia, and Eastern Europe. This leads them to propose a different typology of welfare regimes in developing countries, one in which regional differences among East Asia, Latin American, and East European welfare regimes are prominent.

“Region,” of course, acts as a proxy for myriad other political factors. In an effort to endogenize this variable, Haggard and Kaufman (forthcoming) locate the genesis of the distinct social welfare policy regimes in the political coalitions forged in the mid-twentieth century, at a time when ruling elites responded to the political mobilization of workers and peasants either through strategies of

co-optation or repression. In Latin America, reformist leaders representing the military and the middle classes sought to win electoral support from the urban working class by extending employment-related social insurance. The Cold War produced very different results in Eastern Europe, where Communist dictatorships took control and made workers central to their political strategy, and in Asia, where virulently anti-Communist leaders severely limited the emergence of left parties and weakened labor. Each of these ruling coalitions played a crucial role in setting up each nation's development strategy – whether ISI or EL growth – which reinforced and supported the initial policy choice. Where large urban labor forces were co-opted or integrated into the state in Latin American and Eastern Europe, ISI policies were chosen, further cementing the industrial workforce's privileged place in the economy. In Asia, as many of the nations emerged from colonial rule in the postwar period, Cold War concerns and U.S. support encouraged EL development strategies.

Haggard and Kaufman (forthcoming) represents a significant advance over much of the literature and is arguably the most comprehensive study of social policy variation – across regions, policy areas, and time – in developing countries. It draws on nearly all the major strands of the welfare state literature, both in Europe and in the developing world. Accounting for the variation in the character of social spending across this diverse set of countries and regions is a daunting, if not impossible, endeavor. As a result, Haggard and Kaufman present a causal explanation that is nearly as complex as the reality it describes. The line between theory and history is blurred to the point that it is unclear which variables are more important than others.

Haggard and Kaufman's emphasis on regional clusters of welfare regimes may obscure more than it reveals. On the one hand, the "cross-regional" differences are not as stark as they are portrayed. Consider the relative emphasis on education spending, which, according to these explanations, differs dramatically between East Asia and Latin America. There is in fact no difference in average spending on education as a percentage of GDP between Latin American and Asia – both are 3.2 percent – and the difference in average education spending as a percentage of total government spending is only 2.3 percent (17.5 percent of total spending in Asia versus 15.2 percent in Latin America). Neither difference is statistically significant (p -values of 0.6 and 0.55, respectively). Moreover, the "regional dummies" invoked by the analysis conceal too much within-region variation. Within-region differences are perhaps starker in East Asia than in Latin America (Ramesh 2005). Haggard and Kaufman's explanation for the cross-national variation within regional clusters invokes a long list of potential explanatory variables, which includes regime type, strength of the labor movement, economic structure, strength of populist parties, and so on. Additional quantitative and qualitative tests are necessary to specify the relative importance of these factors and the levels and critical values at which changes in these variables account for cross-national variation within a regional cluster.

The preceding discussion illustrates that our understanding of the variation among social policies in developing countries, and of its political determinants,

is still spotty. While most studies have relied on aggregate spending data, we need to supplement these analyses with information on differences in the design of social policies, including the scope of coverage, the mode of financing, and so on. On the other hand, the proposed explanations for the variation in spending lump together many factors without providing information on the relative importance of each variable. At this point, these studies do not yet provide empirical support for the critical causal steps or micro-logic of their explanation. To take only one example, while these studies invoke the role of employers as important players shaping the choice of economic and social policies, they present no evidence on the preferences of producers in countries pursuing either ISI- or EL-based strategies. As research on social policies in developing countries enters a mature stage, future studies need to trace out the causal processes that are implied by existing theoretical explanations.

STABILITY AND CHANGE IN SOCIAL POLICIES

Beginning in the mid-1970s, welfare states throughout the world entered a turbulent period of transformation. Change was common in both developed and developing countries. In advanced industrialized economies, governments attempted to reconcile their commitment to social protection with new economic and demographic constraints. However, the magnitude of change experienced by advanced industrialized economies pales in comparison to the thoroughgoing policy changes that would occur in Latin America, Asia, and Eastern Europe in the 1990s. In some cases, these changes involved the privatization of risks. In other cases, governments attempted to redress the distributional biases of existing policies, either through the introduction of universalistic programs or of policies targeted at the poor.

Considerable debate exists on how to characterize the current transformations. Some studies suggest that welfare states have entered a new era, one of “permanent austerity” (Pierson 1998). The “new politics” of the welfare state is characterized by the efforts of politicians to impose unpopular welfare cuts on their electorates or on powerful groups that benefited from existing policies (Pierson 1994; Weaver 1986). Other scholars suggest that the dichotomy between expansion and retrenchment is too sharp, as politicians expand benefits to some constituencies to avoid political blame for unpopular cuts. Recent adjustments, these scholars argue, also include “subterranean transformations” brought about by the actions of government program administrators, nongovernment providers of benefits, or employers (Hacker 2004). These lead to the internal adaptation of existing policies to new, changed purposes (a process referred to as “policy conversion”) or to the “layering” of new policies on the old ones (Hacker 2004; cf. Thelen and Streeck 2005; Hacker 2004).

A number of studies have argued that institutional differences among liberal, conservative, and social democratic welfare regimes remain predictors of the adjustment of institutions of social protection during this period (Scharpf and Schmidt 2000; Huber and Stephens 2001). Across advanced industrialized

countries, existing social policies have been subjected to common pressures, which include population aging and the decline in manufacturing employment. We find, however, strong divergence in the evolution of policies. Scandinavian countries have been able to maintain a relatively egalitarian wage structure and have expanded their social services. Countries with conservative welfare regimes have been unable to respond to the decline in manufacturing employment through a similar expansion of public sector employment and have been trapped in the equilibrium of low labor force participation rates and rising payroll taxes. By contrast, countries with liberal welfare regimes have deregulated labor markets and reduced existing entitlements. While these policy changes have facilitated an expansion of low-productivity private-sector employment, this has come about at the price of rising income inequality (Esping-Andersen 1996; Iversen and Wren 1997).

Haggard and Kaufman hypothesize that welfare regimes influence the trajectories of reform outside the core of advanced industrialized societies (Haggard and Kaufman forthcoming). Both Eastern European and Latin American countries experienced prolonged periods of stagnation during the “lost decade” of the 1980s, which eroded the fiscal basis of their welfare states. In response to these fiscal pressures, Latin American governments introduced more dramatic program cuts than East European countries. Haggard and Kaufman attribute the higher resilience of social policies in Eastern Europe to the broader scope of preexisting policies, which generated widespread expectations of publicly provided benefits and services among citizens and support for these programs by more powerful groups.

Explanations stressing the causal importance of welfare regimes are problematic due to the high levels of aggregation of their central explanatory variable. While they provide an accurate *description* of macro-level differences among countries – such as differences in labor force participation rates, levels of public sector employment, and so on – which are affected by preexisting social policies, these studies rarely present a compelling political explanation of these outcomes. The line between description and explanation is further blurred by the incorporation of numerous explanatory variables in addition to a welfare regime, which, in itself, is already a highly aggregated variable. These include the partisan preferences of government, the number of veto players, the centralization of the wage bargaining system, the strength of organizations representing women, and so on (Huber and Stephens 2001).

These explanations are also not equipped to account for *within-country* variation in the trajectory of reforms. This variation is an enduring aspect of the recent politics of welfare state adjustment. An early study of policy retrenchment in the United States and the United Kingdom found that “there is more variation in the outcomes among particular programs than there is between the overall records of the two countries” (Pierson 1994: 131). In both countries, conservative politicians experienced their greatest success in reducing the scope of public housing programs (Pierson 1994: 142). By contrast, programs such as health care and disability proved to be more resilient, weathering conservative

attacks. The recent politics of social policy reform in developing countries is also characterized by marked variation across policy areas – sometimes involving dramatic expansion of policies and cutbacks of other programs (Mares 2008). Consider the following examples. While Mexico has privatized its pension system, it has also introduced a policy providing medical benefits to all mothers. Taiwan has introduced universal health insurance while creating a pension program based on individual retirement accounts. These examples contradict the hypothesis that East Asian welfare regimes have expanded while Latin American welfare regimes have retrenched. Moreover, they show that region-dummies or country-specific variables cannot account for the observed empirical variation in social policy trajectories in the developing world.

Pierson's explanation for the variation across policy areas has invoked the consequences of previous programs, arguing that existing policies "lock in" subsequent developments (Pierson 1993, 1994). One important theoretical contribution of the literature on social policy retrenchment is its careful disaggregation of the political mechanisms by which previous policies "create politics" (Pierson 1993, 1994). Preexisting policies, these studies argue, affect the strength and mobilization of interest groups as well as the ability of policy entrepreneurs to help latent groups overcome their collective action problems (Pierson 1993, 1994). Other studies have shown how the design of different programs (means tested versus insurance) affects recipients' attitude toward government and differences in political participation (Soss 2000; Campbell 2003). Still other studies have argued that preexisting policies influence the cognitive resources of policymakers and the solutions available during periods of crisis (Heclo 1974).

What are the exact features of previous policies that matter? Studies have contended that two features of policy design are likely to be consequential. The first is the size of a program. Pierson (1994) argued that larger public programs lock in large constituencies, which results in greater stability. The second feature is the maturity of a program, which raises the costs of shifting to an alternative path. In a study of pension reforms in advanced industrialized societies, Myles and Pierson argued that pension systems financed on a pay-as-you-go basis raise "insurmountable" barriers to reform due to the fact that a shift to a prefunded policy imposes on working generations the burden of paying both for their own benefits and for those of future retirees (Myles and Pierson 2001: 313).

The recent wave of reforms adopted by many countries around the world provides an ample testing ground for these explanations. It is not clear at this point whether explanations developed for OECD countries can account for social policy trajectories in other regions. In many countries, the largest public programs – pensions – have experienced the most dramatic change, while smaller programs have proven to be more resilient. By 2005, 24 countries around the world had reformed their public pension system, adopting a private pillar. It thus appears that the "double-payment" problem has not exerted the same constraining effects identified by Myles and Pierson (2001) in the context of advanced industrialized countries. In addition, these developments call into

question the generalizability of the political logic formulated by Myles and Pierson to capture the dynamics of social policy reform in a period of austerity. At the center of this explanation is the assumption that voters prefer the policy status quo over other alternatives. Blame-avoiding politicians try either to obfuscate these measures or to diffuse the costs of reform by imposing them on broader segments of the population. The assumption that pivotal voters are irrevocably committed to the policy status quo does not travel well outside the OECD context. In many countries around the world that have privatized their pension systems, most of the population distrusted the ability of the state to finance future welfare state commitments and supported some kind of reform of the public pillar. What made these dramatic reforms possible was precisely the defection of pivotal groups from the policy status quo.

These observations suggest a number of areas for future research. On the one hand, studies to date have been unable to isolate the specific features of existing policies that generate stronger lock-in effects. In looking for plausible alternatives, future studies need to pay attention not just to institutional differences in the design of benefits but also to institutional differences that affect the “credibility” of a social program and the beliefs held by citizens that the state will honor future social policy commitments (Mares and Carnes forthcoming). On the other hand, future studies need to explain why the coalitions supporting different policies vary so much across policy areas. Why have voters simultaneously endorsed the privatization of pension benefits and the introduction of health benefits targeting the poor? The exploration of these questions in future studies will add stronger foundations to existing institutional explanations that stress the importance of policy feedbacks.

CONCLUSION

Peter Lindert once noted the “Robin Hood paradox” of social spending: “Redistribution from the rich to the poor is least present when and where it seems most needed” (Lindert 2004a: 15). Ironically, the comparative literature on social protection has exhibited a similar paradox for several decades. It has generated immense knowledge about social protection in the richest countries of the world and too little about cases where social policy can have the biggest impact. Research on Scandinavian welfare states can fill up entire libraries. Yet, at the same time, we lack basic descriptive indicators on the scope of social protection in many developing countries.

This chapter has documented an ongoing change in the literature. By undertaking research that bridges across regions, a number of studies have begun to close the gap in understanding the differences in social policy between developed and developing countries. Overcoming problems of multicollinearity that have plagued earlier research, these studies have subjected central theoretical explanations of the determinants of social spending to more systematic tests. Insights that have been developed to account for developed countries – such as the existence of a positive relationship between economic openness and

larger welfare states – appear to be robust. Other studies have tried to identify the political coalitions that have given rise to welfare regimes in developing countries and have shown how differences in the structural endowments of these economies facilitate the formation of these coalitions. The most successful explanations are those that establish compelling theoretical and empirical links between macro-level (structural or institutional) variables and the social policy preferences of key political players, on the one hand, and ordinary citizens, on the other. Finally, this chapter has identified areas of research – such as the study of social policy retrenchment – where recent cross-regional research has partially disconfirmed some earlier results found in advanced industrialized economies alone. Lock-in effects of pre-existing programs are less enduring than previously assumed.

But many challenges lie ahead. Many of our findings concerning social protection in developing countries are still based on an analysis of aggregate spending data, which is an imperfect metric of differences in the scope of social protection. More systematic data collection on the implementation of policies, the level of coverage, and the level of benefits is needed. So far, existing studies have provided broad-brush accounts of the political processes leading to distinct types of spending in the developing world. They need to be supplemented by additional historical research tracing out the political processes outlined by these studies and testing whether key assumptions about the policy preferences of different groups made by these studies are correct. While we have begun to confront the Robin Hood paradox of the literature on social spending, we are still a long distance away from resolving it.

Making Causal Claims about the Effect of “Ethnicity”

Kanchan Chandra

An investigation into the effects of which “ethnicity” may be a cause is among the fastest-growing research agendas in comparative politics today.¹ But causal claims about the effect of ethnicity in the discipline have so far been made without reference to concepts. As comparative political scientists, we talk about what ethnicity *does* while saying little about what ethnic identity *is*. We agree roughly on which identities we *classify* as ethnic. But we do not have a definition of ethnic identities that captures our classification and do not seem to believe that we need one. Many of us who theorize about the effect of ethnic identity proceed without a definition. Those who have provided definitions, including myself in previous work, propose theories and employ classifications that are disconnected from or inconsistent with them (Horowitz 1985; Fearon 2003; Chandra 2004).² And, while we now have a large literature that criticizes existing data, measures, and theories about ethnicity, criticisms of the lack of concepts from research in this field are also largely absent.

This chapter places causal theorizing about ethnic identity on a conceptual foundation for the first time.³ It proposes a definition of ethnic identity that captures the classification of ethnic identities to which our causal claims refer and eliminates definitions that do not. It uses this definition to identify properties that can reasonably be associated with ethnic identity and those that cannot. It argues, by evaluating previous causal claims against this conceptual foundation, that most are not reasonable *even when evaluated on their own terms*. And finally, it proposes new conceptually driven criteria that might serve as the basis of more reasonable causal claims.

¹ This chapter draws on material from “What Is Ethnic Identity and Does it Matter?” published in the *Annual Review of Political Science* (2006) and a book manuscript, titled *Ethnicity, Politics and Economics* which introduces a set of concepts for thinking about ethnic identity and uses them to theorize about political and economic outcomes generally.

² See Chandra (2006) and Chandra and Wilkinson (2008) for elaboration.

³ In other work, I also argue that we cannot do empirical work on the effect of ethnicity without reference to concepts (Chandra and Wilkinson 2008). But here, my focus is on theorizing rather than empirical verification.

By a "reasonable" claim, I mean a claim that gives us some reason to believe that the properties it attributes to ethnic identity are associated uniquely or disproportionately with *ethnic* identity rather than with any identity, ethnic or otherwise. Absent such properties, we have no reason to think about it as a claim about ethnic identity at all, rather than as a claim about the effect of any identity, ethnic or otherwise. "Reasonableness" is different from and prior to the question of empirical verification. It refers to presumption rather than proof (see Zuckerman this volume). Many reasonable claims can turn out to be wrong when subjected to verification. Others may not be verifiable given existing data and methods. But there is little to be gained by setting out to prove or disprove a claim that we have no good reason to believe.

The reasonableness of causal claims about ethnicity, in turn, depends upon a definition of the term. Every causal claim takes the following form: Ethnic groups are distinguished by property X (such as a common history or a common culture or a common territory). Property X produces outcome Y (such as patronage or civil war or riots). Therefore, ethnicity produces outcome Y. A definition provides the basis for associating some property X (whatever it might be) with ethnic identity and therefore making reasonable claims about the effect of that family of identities.

To illustrate, consider an example from a different field. Suppose I claim that democracy causes economic development by virtue of its ability to provide secure property rights. A commonly accepted definition of democratic government is "government by rule of law." Rule of law implies secure property rights. Consequently, this claim seems on the face of it to be reasonable, although it may well turn out to be false upon verification. Now suppose I claim that democracy causes economic development by providing territorially based government. There is nothing in the range of definitions of this concept that indicates that the property of territorially based government is uniquely or disproportionately related to democracy. This property appears to be a feature of all modern states. Consequently, this claim is *prima facie* unreasonable.

This chapter proposes the following definition of ethnic identities: Ethnic identities are an *arbitrary subset of categories in which descent-based attributes are necessary for membership*. I say an "arbitrary" subset because, while we can list the restrictions that wall off the subset of identities that we classify as ethnic identities from the larger subset of descent-based identities, these restrictions have not so far been shown to have analytical value. "Nominal" ethnic identities are those ethnic identity categories in which an individual is eligible for membership based on the attributes she or he possesses. "Activated" ethnic identities are those ethnic identity categories in which she or he professes membership or to which she or he is assigned by others as a member.

The principal innovation in this definition, introduced in the distinction between categories and attributes, is in how it specifies the role of descent. Virtually all social science definitions of ethnic identity emphasize the role of descent. But they do so differently, using one of the following five characteristics: (1) a common ancestry, (2) a myth of common ancestry, (3) a common

place of origin, (4) a myth of a common place of origin, or (5) a “group” descent rule for membership, according to which an ethnic group is one that is defined by a membership rule of the following form: You are coded as a member of group A if your parents were also coded as As (Fearon and Laitin 2000a). These definitions also combine descent with other features, treating one or more of the following as additional defining characteristics: (6) a common culture, (7) a common language, (8) a common history, (9) a common territory, or (10) conceptual autonomy. This definition, by contrast, is a “minimalist” one, which eliminates all properties other than descent.

The criterion I use to justify this definition is that it captures the conventional classification of ethnic identities within comparative politics to a greater degree than the alternatives. In other work, I evaluate in some detail the match between each definition and this conventional classification (Chandra 2006, 2007). This chapter summarizes the results of that evaluation. I employ this criterion in order to evaluate our previous causal claims on their own terms. Since these claims draw on and are interpreted as applying to the conventional classification of ethnic identities, evaluating them on their own terms requires us to identify a definition that captures the identities to which they refer.

Building on the definition proposed here and the elimination of the alternatives, I argue that there are so far only two intrinsic properties that we can associate with ethnic identities on average: (1) “constrained change” in the short term and (2) “visibility.” These properties are logically derived from two average properties of descent-based attributes: stickiness and visibility. There may be other such properties implicit in the definition that remain to be identified. But there are also many properties that we *cannot* associate with ethnic identities by definition – at least with ethnic identities as we classify them. These properties are of three types: (1) the 10 properties identified above by definitions that do not capture our classifications; (2) 2 properties – “fixedness” and “exogeneity” – that contradict the definition that does capture this classification; and (3) several additional properties such as “norms, networks, and institutions,” that are neither associated with ethnic identities in any definition nor justified by a cursory look at the data.

When placed and evaluated against this conceptual foundation, our presumptions about the causal effects of ethnicity in comparative politics turn out to be extraordinarily weak. Most of our claims rest on properties that cannot intrinsically be associated with the identities that they are intended to describe. Consequently, they are too weak to make them worthy of verification. Weak presumptions, furthermore, generate unreasonable questions as well as unreasonable hypotheses. The cost we have paid in theorizing about ethnicity independently of a conceptual foundation, then, has been not only in the quality of the answers we have produced but in the quality of the questions we have asked – and the questions we have not asked and not imagined.

Moving forward, the conceptual arguments made here provide a basis on which to build more reasonable causal claims and generate new questions about ethnicity. They suggest three guidelines: (1) Our claims and questions should disaggregate the concept of ethnicity. The effect of ethnicity on some outcome

of interest should be traced, not as one big causal path originating from one big concept but through several causal paths, possibly interdependent, originating from several narrow concepts (see also Rodden this volume; Huckfeldt this volume; Zuckerman this volume). (2) Our claims and questions about the effect of one or more of these concepts should rest on the properties of constrained change and visibility – and not on properties such as a common culture, network ties, institutional affiliations, and so on, which cannot be justified by a conceptual argument. While this guideline closes the door on some claims, it opens new doors to new claims and questions. (3) We should think in terms of distributions, not dichotomies, in making these claims and raising new questions.

The impetus for placing causal theorizing about ethnic identity on a conceptual foundation indeed comes from a clash of two paradigms within the field of research on ethnic identity – primordialism and constructivism. “Primordialism” – a way of thinking about ethnic identities as fixed and exogenous to human action – was the older paradigm, which dominated the study of ethnic identity as an independent variable. “Constructivism” – a way of thinking about ethnic identities as fluid and endogenous to human action – was a new paradigm that dominated, indeed initiated, the study of ethnic identity as a dependent variable (see Lichbach this volume; Ross this volume).⁴ In earlier work and in the broad project of which this chapter is a part, I argued that cumulative progress in the study of ethnic politics requires us to switch paradigms in the study of ethnic identity as an independent variable – that is, to incorporate assumptions of fluidity and endogeneity in our causal statements about ethnic identity (Chandra 2001, 2005, 2007).

On closer inspection, however, the nonincorporation of constructivism into causal claims about ethnic identity turned out to be simply a symptom of a much deeper problem to which constructivism is itself not immune: the lack of a definition of an ethnic identity. The two paradigms – primordialism and constructivism – can be reduced to statements about the properties that can reasonably be associated with ethnic identity. Primordialism associates the properties of fixity and exogenousness with ethnic identity and constructivism with the properties of fluidity and endogeneity. In deciding which of these two approaches is reasonable, we require some prior statement of what ethnic identity is.

For example, if we believe that ethnic identities are identities defined by some objective rule determining common ancestry (if that can be deemed to

⁴ Note, however, that the term “constructivism” in the study of ethnic politics is different in meaning from the term “constructivism” as it is used in the study of states and international relations, described in Solingen’s and Lichbach’s chapters in this book. At a very broad level, the term constructivism refers to the position that some things that we take to be natural are in fact the products of an ideational construction. Constructivism in the field of ethnic identities and in the study of states’ international relations shares that position. But the things we focus on as being constructed are different. In the study of international relations and states, the term constructivism has come to refer primarily to the construction of the *interests* of existing states rather than their boundaries. In the study of ethnic identities, the term refers principally to the way in which the *boundaries* of ethnic groups are constructed, in addition to their interests.

exist), then it indeed makes sense to assert that ethnic identities are fixed and exogenous. But if we believe that ethnic identities are defined by their membership rules or by the nature of their myths or the possession of a shared history, then it follows that they should have the properties of fluidity and endogeneity. Different definitions, furthermore, would lead us to predict different kinds of fluidity and endogeneity. Incorporating constructivism into the study of ethnic politics, it became clear, requires incorporation of a conceptual foundation into the study of ethnic politics.

However, constructivism as it applies to the study of ethnic politics does not fall neatly into any of the three paradigms – culture, structure, and rationality – identified in this book as defining the study of comparative politics. It sprawls across them, including and discarding elements from all three. Given a very broad definition of “culture” as some collectively shared or proposed system of interpretation, we can surely think about ethnic identities as cultural artifacts.⁵ But these interpretations are imposed on and interact with existing structures. And the process by which these interpretations are constructed often involves rational choices, although it need not always. Thus, while paradigms have certainly determined the direction of research in this field and must be redefined in order to redirect it, they cannot be categorized in the same way as the paradigms that define this book.

The first section of this chapter identifies the properties associated with ethnicity in our previous causal claims. The second section elaborates on why these claims require a definition in order to be considered reasonable. The third section outlines the definition proposed here and its relationship to our conventional classification of ethnic identities. The fourth section justifies this definition by evaluating it against a sample of identities that comparative political scientists routinely classify as ethnic. The fifth section evaluates this definition against the alternatives using the same criterion. The sixth section identifies the properties of ethnic identities derived from the earlier discussion on definitions – and lists the properties that we cannot reasonably associate with ethnic identity. The seventh section uses the properties identified – and eliminated – to evaluate some of our causal claims about the effect of ethnic identity. The eighth section uses these properties to identify new directions for causal theorizing.

Throughout, I use the term “ethnic group” or “ethnic identity” loosely to mean either a collective – a cluster of individuals with a common purpose – or a category – a cluster of individuals who share only a descriptive label. Both meanings are associated with the word “group” by the *Oxford English Dictionary*, and this ambiguity has led to consequential errors in our theorizing (Brubaker 2004: 8–10). Where a precise interpretation of the term group is consequential, I address the ambiguity explicitly. But in general, in order to evaluate other work on its own terms, I am also forced to use words it uses on

⁵ Note that to say that culture matters in the construction of ethnic identities is very different, as I will argue subsequently, from saying that ethnic identities have a *common* culture.

TABLE 15.1. *Properties Attributed to Ethnic Identity in Our Causal Claims*

Claims	Properties Associated with Ethnic Categories
Causal claims linking some concept related to ethnic identity with violence	Fixedness Common history Distinct emotional responses Dense social networks
Causal claims linking some concept related to ethnic identity with patronage politics	Territorial concentration High cost of change Visibility Networks Norms Institutions
Causal claims linking some concept related to ethnic identity with the destabilization of regimes and/or states	Division of labor Territorial concentration Fixedness

their own terms, with the ambiguities intact. When not evaluating previous work here and elsewhere, I drop the term group altogether and employ a more precise distinction between “nominal” and “activated” categories of membership.

THE PROPERTIES OF ETHNIC IDENTITY ASSUMED BY CAUSAL CLAIMS ABOUT ETHNIC IDENTITY

Table 15.1 summarizes the properties that some of our causal claims ascribe to ethnic identities. This survey is not meant to be exhaustive. I have chosen a handful of examples sufficient only for the purposes of illustration.

Consider, first, the claims linking some concept related to ethnic identity with some form of violence:

- According to one argument, one cause of civil war between ethnic groups is the “security dilemma” introduced by the collapse of the state (Posen 1993a). The reasoning underlying this argument is as follows: The collapse of the state governing a multiethnic society creates an environment analogous to anarchy in the international environment, with ethnic groups analogous to states. In an anarchic environment, ethnic groups, like states, arm themselves out of fear for their own security. But this makes other groups, like other states, more fearful and gives them an incentive to arm themselves also. The result is an increased threat of war, reducing everyone’s security in the long run. War is especially likely if the groups in question have a history of rivalry, since this gives each group more reason to assume the worst of the other.
- This argument assumes, implicitly, that ethnic groups have two properties. First, it implies that, like states, they are fixed entities – for

if individuals could change their ethnic identities, then one response to the collapse of the state might be simply to switch to less threatening identities rather than go to war. Second, it implies that ethnic groups are more likely than other types of groups to have a common history. Otherwise, the security dilemma should be an explanation for intergroup conflict in the wake of state collapse in general rather than ethnic conflict in particular.

- According to a second argument, ethnic violence is motivated by emotions such as fear, hatred, or resentment (Petersen 2002). The initial trigger for such violence, according to this argument, lies also in the collapse of the state. But given this trigger, the target of violence will depend upon the specific emotional response aroused among ethnic groups. Groups motivated by fear will choose as their target those ethnic others who are the greatest threat. Groups motivated by resentment will choose as their target those ethnic others who are farthest up the status hierarchy, whether or not they are the greatest threat. And groups motivated by hatred will target those ethnic others with whom they have battled in the past, regardless of their threat potential and their position on the status hierarchy. If this argument is to be read, as it is intended, as an argument about *ethnic* violence specifically rather than violence in general, then ethnic groups must have the property of experiencing stronger, or more distinct, emotional reactions in the wake of state collapse than other types of groups.
- A third argument attributes the higher frequency of *interethnic* violence, in comparison to *intraethnic* violence, to the assumed property of network ties. The existence of such ties, according to this argument, makes within-group cooperation easier than across-group cooperation (Fearon and Laitin 1996: 730).

Consider now the family of arguments that posit a causal association between some concept related to ethnic identity and patronage politics:

- The reasoning underlying one such argument goes as follows: The goods of modernity are distributed on a spatial basis. It makes sense for individuals desiring access to these goods, therefore, also to organize on a spatial basis. Ethnic groups are territorially concentrated. The struggle for access to these goods, therefore, is organized on an ethnic basis. This argument explicitly assumes that ethnic groups have the property of territorial concentration: “there is no denying that the members of an ethnic group tend to cluster in space; nor can it be questioned but that colonial policy made every attempt to assign ethnic groups to stable and rigidly defined areas” (Bates 1974: 464).
- A second such argument follows a different logic (Fearon 1999; Caselli and Coleman 2001). Patronage politics, according to this argument, requires exclusive coalitions in which entry of new members is costly. Ethnic identities are costly to change and therefore are one basis on which exclusive coalitions can be built. Consequently, patronage politics leads

individuals to activate ethnic identities. The property this argument turns on is the relatively higher cost associated with changing ethnic rather than nonethnic identities.

- Yet another argument highlights the property of visibility (Chandra 2004). Patronage politics, according to this argument, takes place under severe information constraints. The identities most likely to be activated by patronage politics, therefore, are those that are most visible in a limited- information environment. Ethnic identities are more visible than other types of identities. Consequently, patronage politics activates ethnic identities. The property this argument turns on is the supposed visibility of ethnic identities.
- A fourth argument explicitly designed to distinguish empirically between different mechanisms that might link ethnicity with coethnic favoritism in the distribution of public goods assumes the properties of norms, networks, and institutions (Habyarimana et al. 2007). Coethnics favor each other in the distribution of public goods, according to this argument, because they are more able to locate and sanction each other than noncoethnics. The ability to locate coethnics, the authors argue, is attributed to the supposed existence of network or institutional ties that supposedly bind coethnics, while the willingness to sanction coethnics is attributed to a shared normative framework.

Consider a third family of examples, which link the politicization of ethnic divisions with the destabilization of states or regimes:

- The reasoning underlying one argument linking ethnic identities with the destabilization of states goes as follows: Ethnic groups are “incipient nations” with a territory and a division of labor, which makes them candidates for creating new nations and states. But classes and economic groups are economically specialized and are not associated with territory. Consequently, “economic or class disaffection threatens revolution, but disaffection based on ethnic identities threatens partition, irredentism, or merger, a redrawing of the very limits of the state” (Geertz 1973a: 261). This argument assumes, explicitly, that two properties of ethnic groups are a common territory and functional differentiation. Otherwise, the threat posed by ethnic groups would be no different from the threat posed by class and other economic groups.
- The reasoning underlying another argument, which links ethnic divisions with the destabilization of democracies specifically, goes as follows: Democracies are destabilized by the permanent exclusion of some minority groups from power. Ethnic divisions are fixed. Elections based on ethnic divisions, therefore, produce permanent winners and permanent losers based on ethnic demography. Consequently, the politicization of ethnic divisions threatens democratic stability (Horowitz 1985; Rabushka and Shepsle 1972). The key property of ethnic identity that this argument rests on is fixedness: If ethnic identities are fluid, not fixed, then the other propositions fall through.

- According to another argument in this family, the introduction of democracy in multiethnic societies is likely to lead to conflict for the following reason: Because it is based on the popular vote, democracy empowers economically excluded ethnic majority groups against “market-dominant” minorities, who control a disproportionate share of economic resources (Chua 2003). The conflict between these two groups with opposing interests produces instability, authoritarian backlash, and violence. For this argument to apply to democracy in *multiethnic* societies specifically rather than democracy more generally, there must be some property specific to ethnic identities that brings about this conflict. Unequal distribution of economic resources across social groups is a pervasive fact throughout the democratic world, but not all groups are believed to threaten democracy for this reason. The property that Chua’s argument implicitly relies on is *fixedness* of both identity and wealth. It is only when individuals from poor ethnic categories cannot move, individually or collectively, to richer ones that we should see the predicted outcome.

WHY MAKING REASONABLE CAUSAL CLAIMS ABOUT THE EFFECT OF ETHNIC IDENTITY REQUIRES A DEFINITION

To say that we must first stipulate what a concept is before theorizing about what effect it has seems a truism. But in a field that has long proceeded without a definition, it is a point worth arguing. A popular argument against starting with a definition goes somewhat as follows: “The absence of a definition is not an insurmountable obstacle – it simply makes research less efficient. Without a definition on the basis of which we can weed out unreasonable hypotheses, we will end up testing more than necessary. But ultimately, we will get the same answers by verifying claims not justified by a definition as by verifying claims justified by a definition.”

In response, it is worth highlighting that the cost of proceeding without evaluating our presumptions against a definition lies not simply in inefficiency within an existing research agenda, but in eliminating more fruitful research agendas from consideration. But the argument in favor of proceeding to the data without a definition cannot be sustained even if testing hypotheses within existing research agendas were our sole concern. Let me elaborate by examining both types of new data that we might collect: cross-national data and country-specific data.

The cross-national data sets that we currently have count ethnic identities using criteria that are entirely opaque (Chandra 2008; Chandra and Wilkinson 2008). Thus, even if we found a correlation between a concept related to ethnic identity and the outcome of interest, we would not be able to determine that this correlation verifies the existence of properties assumed in our causal claims such as fixedness, networks, a common history, and so on. We would have to collect new cross-national data on identities that exhibit our presumed property and

then match these data to our data on the identities we classify as ethnic to see if there is an association.

What if we try to develop a reasonable claim by going a group-specific and country-specific route? Why not start with a group that is by general agreement an ethnic group in a given country, examine the properties that facilitate that group’s participation in patronage politics, and abstract from these observations to make reasonable causal claims using these properties? Although this is a common strategy that I have myself employed in previous work (Chandra 2004), it suffers from a serious flaw: In the absence of a definition, we cannot draw the inference that the properties associated with the outcome by the group in question are properties related to the ethnic aspect of that group. Put differently, the problem is this: Abstracting from a “case” to some general proposition requires us to decide what a case is a case *of*. But we cannot decide what a case is a case *of* without a definition.

To illustrate, consider the study by Habyarimana et al. (2007). The authors attempt to identify the mechanisms that link ethnicity to patronage by studying a sample of individuals in Uganda predominantly composed of members of the Baganda group, along with several other groups classified as ethnic in Uganda. They observe that the tendency of Bagandas and members of these other groups to favor each other in the allocation of public goods is based on the existence of network ties, institutional affiliations, and shared norms. From this observation, they draw the inference that network ties, institutional affirmations, and shared norms are likely to explain the link between *ethnic group membership* and the tendency toward in-group favoritism.

Now, there are many other descriptions that might simultaneously fit these groups individually or the set of groups taken together. The Baganda, for instance, are a group associated with a state that preceded the advent of colonial rule, a group associated with a historical kingdom, and a plurality group, among other things. Observing that the Baganda rely on network ties to accomplish some task gives us no reason to believe, in the absence of a definition, that these ties are a property of the ethnic aspect of the Baganda – and therefore abstract from it to make a claim about the effect of ethnicity. We might just as well treat the Baganda as a case of groups with a historical association with a state, and therefore treat network ties or other properties linked with in-group favoritism as characteristics of groups with precolonial kingdoms. This would lead us to abstract from the same observation to make a claim about the relationship between early state development and patronage rather than an observation about the relationship between ethnicity and patronage. Without a definition, the inference from this observation that ethnicity matters is as (un)justifiable as the inference that ethnicity does not matter.

WHAT IS AN ETHNIC IDENTITY?

An ethnic identity, I propose, is one of an arbitrary subset of *categories* in which descent-based *attributes* are necessary to determine eligibility for

membership. I say an “arbitrary” subset because, while we can list the restrictions that wall off the subset of ethnic identities from the larger subset of descent-based identities, these restrictions have not so far been shown to have analytical value.

“Nominal” ethnic identities are those ethnic identity categories in which an individual is eligible for membership based on the attributes she or he possesses. “Activated” ethnic identities are those ethnic categories in which she or he professes membership or to which she or he is assigned by others as a member. All individuals have a “repertoire” of nominal ethnic identity categories. This consists of all the meaningful membership rules that can be fashioned from an individual’s given set of descent-based attributes, with each rule corresponding to a nominal category. The ethnic identity an individual actually activates is chosen from this repertoire.

By “attributes that are necessary to determine eligibility for membership,” I mean either those that qualify an individual for membership in a category or those that signal such membership. By “descent-based attributes,” I mean attributes associated with or believed to be associated with descent. By attributes “associated with descent,” I mean attributes that are acquired through genetic inheritance (e.g., skin color, gender, hair type, eye color, height, and physical features) or through cultural and historical inheritance (e.g., the names, languages, places of birth and origin of one’s parents and ancestors) or acquired in the course of one’s life as markers of such an inheritance (e.g., last name or tribal markings). By attributes “believed to be associated with descent,” I mean attributes around which a credible myth of association with descent has been woven, whether or not such an association exists in fact. The definition thus includes both a subjective and an objective element.

The set of identity categories in which membership is determined by descent-based attributes is large. Ethnic identity categories are a subset of this larger set defined by the following restrictions:

1. They are large enough for membership to be impersonal. This restriction rules out descent-based categories such as “family,” which, although descent-based, is not classified as an ethnic identity by any of these sources. Thus, a category such as “siblings with the last name Brown” would not count as an ethnic category because its members are known and directly related to each other.
2. They constitute a section of a country’s population rather than the whole. This restriction captures our sense of an ethnic identity as *part* of a whole rather than the whole itself. Thus, “German” would not be an ethnic group in a country that is 100 percent German, even if membership in the category German met the other defining conditions here. But “Bavarian” would be an ethnic category in Germany if it met the other defining conditions since Bavarians constitute part of the larger German population – and “German” would be an ethnic category in the United States if it met the other defining conditions, since Germans constitute part of the larger U.S. population.

3. If one sibling is eligible for membership in a category in any given place, then all other siblings are also eligible in that place. This restriction eliminates large numbers of categories, typically based on single attributes distributed arbitrarily across siblings, and retains others, typically based on complex combinations of attributes that characterize all siblings. These include categories such as “women” or “green-eyed people.”
4. The qualifying attributes for membership are restricted to one’s physical features or to the religion, sect, language, dialect, tribe, clan, race, nationality, region, and caste of one’s parents and ancestors. This restriction rules out identity categories such as “English speaker” when knowledge of English is acquired in the course of one’s lifetime. It also rules out a category such as “descendants of Communists” or “descendants of landowners” since the attributes that qualify an individual for membership in that category do not fall in the allowed list.

If some of these restrictions, especially the last, appear arbitrary, they are. Why impose a rule that requires siblings to be equally eligible for membership before a category can be called ethnic? Why allow this particular set of descent-based attributes and not others? I do not offer analytical justifications for these restrictions here – indeed, I do not think that there is a good reason to wall off ethnic identities from other types of descent-based identities. My purpose here is simply to identify those restrictions that we must impose in order to approximate the conventional classification of ethnic identities. Once we have identified these features, we are also in a position to recognize their arbitrariness and discard them where necessary – or at least modify our interpretations to take such arbitrariness into account.

Let me illustrate this definition using the fictionalized example of Helen, imagined from a *mélange* of characters in Mary Waters’ study of West Indian immigrants in New York (Waters 1999). Born on the English-speaking island of Trinidad to parents of African origin, she has dark skin, dark brown eyes, and straight hair. She moved to the United States after obtaining her high school diploma in Trinidad and works there as a food service employee, earning \$25,000 a year. She belongs to, and votes for, the Democratic Party there. She marries a Haitian man in New York and learns to speak French, which is now her primary language of communication with her children and husband. Her brother Derek, who has lighter skin, light brown eyes, and otherwise similar features, has remained in Trinidad. Their parents are well-educated professionals who belong to the People’s National Movement (PNM) in Trinidad. They are Presbyterians, but Helen herself converted to Catholicism after meeting her husband.

According to the preceding definition, Helen’s ethnic identity categories include “Black” (in which the qualifying attribute, according to current norms, is descent from parents of African origin, signaled by attributes such as the color of her skin and physical features) and “West Indian” (in which the qualifying attribute is descent from parents who live in Trinidad, signaled by her accent,

among other attributes). Both of these categories are determined by attributes associated or believed to be associated with descent, and both of them place Helen and Derek in the same categories. Helen's ethnic identity categories also include "African American" (in which membership is determined by the attribute of descent from African parents, skin color, and physical features *in the United States*). Derek, because he stayed in Trinidad, cannot call himself "African American." But if he moved to the United States, he would be eligible for membership in this category, just like his sibling.

An individual's nonethnic identities, by inference, are all those identities outside the set of nominal ethnic identities. Some of these nonethnic identity categories may also require descent-based attributes for membership. But most of the categories we usually recognize as nonethnic are typically based on attributes acquired during one's lifetime. Thus, while ethnic and nonethnic identities do not map directly to the dichotomy between descent-based and non-descent-based identities, to the extent that all ethnic identities are associated with descent, while many nonethnic identities are not, we can say that there is an imperfect correlation between descent and ethnicity.

Helen's nonethnic ethnic identity categories according to this definition, for example, include some identity categories also based on descent-based attributes such as "descendant of PNM supporters" (excluded because it is not based either on physical features or on the language, religion, race, tribe, caste, nationality, and place of origin of her parents), "people with dark brown eyes" (excluded because, while it is based on her physical features, it excludes her sibling, Derek), and "female" (also excluded because it excludes Derek). They also include "Catholic" (determined by conversion, openly acknowledged, during her lifetime), "French speaker" (determined by a language learned during her lifetime rather than her ancestral language), working class (determined by attributes acquired during her lifetime, such as her high school diploma and her job as a food service worker), and Democrat (determined by her joining the Democratic Party during her lifetime).

According to this definition, ethnicity is not one big concept or three, but many tens of narrow ones. These concepts can be grouped into two broad families: ethnic structure and ethnic practice. Ethnic "structure" refers to any concept that describes some aspect of the allowable descent-based attributes that characterize individuals or populations. These include an individual's repertoire of attributes, the repertoire of nominal ethnic identity categories generated from these attributes, the distribution of attribute and category repertoires in a population, and so on. Ethnic "practice" refers to the ethnic identity categories activated by individuals and populations in different contexts. Since different ethnic identity categories can be activated in different contexts, ethnic practice can refer to as many categories as there are contexts. (A fuller description of these concepts and their relationship to each other is given in Chandra and Wilkinson 2008.)

The distinction between ethnic structure and ethnic practice is consistent with distinctions others have made between "commonsensically real" and

“mobilized” ethnic identities, “latent” and “active” identities, and so on (Fearon 2003; Posner 2005). But it differs in the extent to which the family of concepts under each label is defined and disaggregated, and in the logical linking of each concept in a single integrated framework. This disaggregation – and the logical connection of individual concepts – is critical for how we should construct causal claims about ethnic identity, and I will return to it in the conclusion.

JUSTIFICATION

Since the publication of Donald Horowitz’s *Ethnic Groups in Conflict* (1985), we have come to agree that ethnic identities include *some* identity categories associated with one or more of the following types: religion, sect, language, dialect, tribe, clan, race, physical differences, nationality, region, and caste.⁶ In order to evaluate the match between this definition, alternative definitions, and our classification, I start with a sample from this conventional classification that consists of the following sets of categories:

(Black and White) in the United States;
 (Serb and Croat) in the former Yugoslavia;
 (Mohajir, Punjabi, Pathan, and Baluch) in Pakistan;
 (Flemish and Walloon) in Belgium;
 (Aymara and Quechua) in Bolivia;
 (Yoruba, Ibo, Hausa-Fulani) in Nigeria; and
 (Zulu, Xhosa, and Coloured) in South Africa.

The categories represent agreement among the majority of political scientists on which identities should be counted as ethnic: They are classified as ethnic by at least three of the five principal comparative sources on ethnic groups: Horowitz (1971, 1985), *Atlas Narodov Mira* (1964), Alesina et al. (2003), Fearon (2003), and the *Minorities at Risk Database* (2006) and, by implication, the body of work that draws on these sources. Chosen from across continents – North America, Eastern Europe, Western Europe, Asia, Latin America, and Africa – they are also widely representative. If we can find a definition that covers all the identities contained in this sample, we can have some confidence that it does not refer to a specific geographic area and should cover the identities contained in the population of ethnic identities. Conversely, if a definition does not cover even the identities contained in this sample, on which there is general agreement, then we can be confident that it will not cover other identities contained in the population of ethnic identities.

In evaluating the match between this definition and our classification, I ask three questions of each category in each set in the sample: (1) Do descent-based

⁶ See Horowitz (1985: 51–54) for a comparable but not identical list. The modifications of this list (e.g., the inclusion of identities based on dialect) are based on examining his actual classification of identities as ethnic.

membership rules (subject to the preceding restrictions) distinguish categories in the same set from each other? (2) Do descent-based membership rules (subject to the preceding restrictions) distinguish ethnic categories from the categories we classify as nonethnic? (3) Is a descent-based membership rule (subject to the preceding restrictions) sufficient to constitute the group or does it describe a group that is constituted on some other basis? If it is not sufficient to constitute the group, then it cannot be a *defining* characteristic.

I infer the membership rules from descriptions of the groups in the *Encyclopedia Britannica* and supplement these descriptions with additional sources where cited. Membership criteria are always socially constructed – and there can often be multiple and contested criteria over time. Rather than defend the substance of any particular membership rule, therefore, I probe only for whether it is reasonable to infer, from the description of the group, that *some* descent-based attributes (subject to the preceding restrictions) are necessary for membership in these categories, and that different combinations of descent-based attributes (subject to the preceding restrictions) are necessary for membership in different categories. There may well be additional descent-based or non-descent-based attributes required for membership that I do not identify here – and those that I do identify may well fluctuate in their relative importance over time.

In the United States, commonly accepted criteria for membership in the category “Black” is the attribute of descent from even a single ancestor who migrated to the United States from territories in present-day Africa (the so-called one-drop rule) and/or the possession of certain physical features, such as dark skin and hair, which may be qualifying criteria in themselves or simply signals that indicate the presence of the earlier qualifying criterion (Nobles 2000). Membership criteria for the category “White,” by contrast, require that all of an individual’s ancestors migrated from territories in present-day Europe and/or the possession of some combination of physical features including light skin and hair. Although these criteria do not parallel present-day census criteria, they reflect the weight of previous census and legal classifications.

In the former Yugoslavia, the attribute of descent from one of a number of Slavic tribes that settled in an area inland from the Dalmatian coast and were associated with a state established in 850 A.D. is necessary to be called a “Serb.” To be called a “Croat,” in turn, it is necessary to possess the attribute of descent from other Slavic tribes who settled in an adjacent region in northern Dalmatia and were associated with a state established by Tomislav in 950 A.D. These membership rules can also be equivalently expressed through some combination of descent-based attributes including name, physical features, ancestral language (Cyrillic script for Serbs and Latin script for Croats), and religion (Eastern Christianity for Serbs and Western Christianity for Croats).

In present-day Pakistan, to be eligible for membership in the category “Mohajir,” one must be born in present-day India or be descended from ancestors born in present-day India. This rule can be equivalently expressed

using some combination of name, physical features, or ancestral association with Urdu, all of which signal such descent. To be eligible for membership in the category “Pathan,” one must possess the attribute of descent from one of a group of tribes who migrated to parts of present-day Pakistan and Afghanistan between the thirteenth and sixteenth centuries. This rule can be equivalently expressed using some combination of name, physical features, or ancestral association with one of several dialects associated with these tribes, given the common label of “Pashto,” all of which signal such descent. To be eligible for membership in the category “Baluchi,” one must possess the attribute of descent from one of a number of tribes who migrated into present-day Pakistan, Iran, Afghanistan, and Punjab from their original homeland on the Iranian plateau. This rule can be equivalently expressed using some combination of name, physical features, or ancestral association with the range of dialects given the common label “Baluchi,” all of which signal such descent. And finally, to be eligible for membership in the category “Punjabi,” one must possess the attribute of descent from those who inhabited the territory of undivided Punjab, on either side of the national border, for some unspecified number of generations. This rule can be equivalently expressed using some combination of name, physical features, or ancestral association with the number of dialects spoken in this region, grouped under the common label “Punjabi,” all of which signal such descent.

In present-day Belgium, to be eligible for membership in the category “Flemish” (“Walloon”), it is necessary to possess the attribute of descent ancestors who inhabited the northern (southern) region of Belgium or spoke one of the dialects associated with Flemish (Walloon) for some unspecified number of generations before the twenty-first century (Zolberg 1974). This membership rule can be equivalently expressed through attributes that signal such descent, including name, physical features, and so on.

In present-day Bolivia, in order to be eligible for membership in the category “Aymara,” it is necessary to possess the attribute of descent from ancestors from some set of tribes who inhabited the northern and central Altiplano from at least the fifteenth century. In order to be eligible for membership in the category “Quechua,” it is necessary to possess the attribute of descent from ancestors from some set of tribes who inhabited the southern part of the Altiplano from at least the fifteenth century. Both rules may be equivalently expressed as combinations of attributes that signal such descent, including name, physical features, and an ancestral association with languages or dialects spoken by the tribes labeled “Aymara” or with the languages or dialects spoken by the tribes labeled “Quechua.”

In present-day Nigeria, to be eligible for membership in the category “Yoruba,” it is necessary to have the attribute of descent from the inhabitants of a number of city-states in southwestern Nigeria, speaking related languages, who came to be associated with the common label “Yoruba” in the mid-nineteenth century (Young 1976; Laitin 1986). To be eligible for membership in the category “Ibo,” it is necessary to have the attribute of descent from a collection of lineage-based groups who settled in eastern Nigeria, spoke

related languages, and came to acquire the common label “Ibo” somewhere between the time of the slave trade and the mid-twentieth century (see also Young 1976). The term “Hausa” refers to the inhabitants of a series of city-states in northern Nigeria who came to acquire the common label “Hausa” sometime during or before the nineteenth century. The term “Fulani” refers to nomadic groups dispersed throughout West Africa, some of whom conquered the Hausa city-states in the nineteenth century and settled in them as a ruling class, intermarrying with and adopting the dialects of local inhabitants. The category “Hausa-Fulani” came about in the twentieth century as an amalgam of the categories “Hausa” and “Fulani.” To be eligible for membership in the category “Hausa-Fulani,” it is necessary to have the attribute of descent from ancestors who were labeled “Hausa,” “Fulani,” or both. The rules for membership in each of these categories may be equivalently expressed as combinations of attributes that signal such descent, including name, physical features, and an ancestral association with languages or dialects labeled “Yoruba,” “Ibo,” or “Hausa.”

Finally, in South Africa, to be eligible for membership in the category “Zulu,” it is necessary to possess the attribute of descent from a collection of clans associated with the nineteenth-century kingdom led by Shaka, who spoke related dialects that came to be grouped under the common label of a Zulu language. Xhosas share the attribute of descent from other clans settled in a common region and speaking related languages that came to acquire the common label “Xhosa” at around the same period. And “Coloureds” are those who have the attribute of descent from the Khoisan (whose ancestors settled in parts of South Africa by 150 B.C.), slaves imported by the Dutch, Europeans, and later migrants from East Africa belonging to the Bantu language group. The rules for membership in each of these categories may be equivalently expressed as combinations of attributes that signal such descent, including name, physical features, and, in the case of Zulus and Xhosas, an ancestral association with a set of related dialects or languages.

In all these examples, a descent-based membership rule can safely be taken as a defining characteristic. Take, for instance, a rule for membership in the category “Black” in the United States: In order to be a member of this category, it is necessary to possess the attribute of descent from even a single ancestor who migrated to the United States from territories in present-day Africa. This rule is logically sufficient to constitute a category. There is nothing in it that assumes or requires the prior existence of that category. I will return to this distinction in the discussion of alternative definitions that follows.

While membership in all these categories is determined at least in part by descent-based attributes, we know that descent-based attributes do not distinguish these categories alone. As I noted earlier, they also distinguish identities that we do not conventionally classify as ethnic, such as “descendants of land-owners,” “descendants of Communists,” “aristocrats,” “women,” and “green-eyed people.” The four additional restrictions identified earlier are necessary to approximate our classification of ethnic identities by eliminating these “extras.”

Even with these restrictions, however, the definition probably captures some extra categories that are not conventionally counted as ethnic. Nevertheless, the inclusion of some identities that we do not count as ethnic is less consequential for our purposes than the exclusion from the definition of identities that we do count as ethnic. A definition that covers most of the categories that we call ethnic allows us to evaluate claims about how ethnic identity matters even if it includes some extra categories, since it describes, at a minimum, the entire sample from which the inferences are drawn. But a definition that excludes categories that we routinely classify as ethnic makes us less able to evaluate these claims because it describes a truncated sample.

COMPARISON WITH OTHER DEFINITIONS

The most widely used definitions of ethnic identity proposed in previous literature include the following:

- According to Max Weber, “ethnic groups are those human groups that entertain a subjective belief in their common descent because of similarities of physical type or of customs or both, or because of memories of colonization or migration; this belief must be important for the propagation of group formation; conversely, it does not matter whether or not an objective blood relationship exists” (Hutchinson and Smith 1996: 35).
- According to Donald Horowitz, “Ethnicity is based on a myth of collective ancestry, which usually carries with it traits believed to be innate. Some notion of ascription, however diluted, and affinity deriving from it are inseparable from the concept of ethnicity” (Horowitz 1985: 52).
- According to Fearon and Laitin, an ethnic group is “a group larger than a family for which membership is reckoned primarily by descent, is conceptually autonomous, and has a conventionally recognized “natural history” as a group (Fearon and Laitin 2000a: 20). By “membership reckoned primarily by descent” Fearon and Laitin mean the following: An individual is coded as a member of group A if her parents were also coded as members of that group. I call this the “group descent rule for membership” to distinguish it from the “attribute descent rule” required by the definition I introduced in the previous section.
- In a subsequent refinement, Fearon defines a “prototypical” ethnic group as one that has as many of the following features as possible: (1) membership is reckoned primarily by descent (presumably by the descent-rule mentioned earlier); (2) members are conscious of group membership; (3) members share distinguishing cultural features; (4) these cultural features are valued by a majority of members; (5) the group has or remembers a homeland; (6) the group has a shared history as a group that is “not wholly manufactured but has some basis in fact” (Fearon 2003: 7).
- According to Anthony Smith, an ethnic group is “a named human population with myths of common ancestry, shared historical memories, one or more elements of a common culture, a link with a homeland and a sense of solidarity” (Hutchinson and Smith 1996: 6).

Virtually all definitions in this inventory agree that descent is somehow important in defining an ethnic group. The differences are over how precisely to specify the role of descent, and whether and how other features should be combined with it in defining ethnic groups. The role of descent is specified in five different ways: (1) a common ancestry or (2) a myth of common ancestry or (3) a common region of origin or (4) a myth of a common region of origin and (5) a group descent rule for membership. The features combined with descent are (6) a common culture, (7) a common language, (8) a common history, (9) a common territory, and (10) conceptual autonomy.

The specifications of the role of descent in these definitions differ from the specification I have proposed in subtle but significant ways. Each specification of the role of descent leads us to associate very different sets of properties with ethnic identities – and thus is the basis for substantially different research agendas. The significance of these differences will become apparent when we consider the properties that we can associate with ethnic identity by definition. Here I simply outline these differences.

According to the definition I propose, ancestry, or a myth of ancestry, is critical to the definition of an ethnic group, but *common* ancestry, or a myth of *common* ancestry, is not. For instance, the membership rule for classification of individuals as Black or White in the United States separates individuals who have, or are believed to have, some African blood from individuals who do not, or are not believed to, have a drop of African blood (Nobles 2000). This membership rule makes the categories “Black” and “White” ethnic categories because it requires the possession of attributes based on ancestry (the proportion of African blood) even though it does not require common ancestry.

Further, according to my definition, the region in which an individual’s ancestors resided, or were believed to reside, can define an ethnic identity – but a common region of origin or a myth of a common region of origin is not necessary. Thus, the category “immigrant” in Germany is an ethnic category by this definition, even though those who are classified as immigrants may be descended from ancestors in many different regions, including Turkey, India, and Afghanistan, and be perceived as such. Further, the region in which one’s ancestors resided is simply one of the several descent-based attributes that might define an ethnic identity, not the only one – we can have identities that we count as ethnic that have nothing to do with a region of origin. Thus, the identities “Christian” and “Muslim” in Lebanon would be classified as ethnic identities based on attributes such as descent from parents of a particular religious persuasion rather than a myth of a common region of origin.

Finally, the “attribute-descent rule” logically encompasses the “group-descent rule” for membership: The group memberships of parents and ancestors variously defined become part of the stock of attributes inherited by their descendants and thus the basis for their own ethnic coding. But there are many identities that we think of as ethnic, which are not captured by the group descent rule that the attribute-descent rule does indeed capture. Thus, an individual can be a member of ethnic categories based on attributes that characterize her but not

her parents (e.g., her skin color or mixed descent) as well as categories that characterize her but not her parents (e.g., new categories invented in her lifetime, for which eligibility for membership depends on the attributes they possessed).

I evaluate these definitions and their match with our classification of ethnic identities in some detail elsewhere using the same criteria I use for my own definition (Chandra 2006, 2007). For analytical clarity, I consider each characteristic employed in each definition separately. Then I ask the same three questions of each defining characteristic that I asked of my own definition: (1) Does this characteristic distinguish ethnic categories included in the sample from others in the same set? (This requires us to ascertain both that each category in each set has this characteristic in common and that different categories in the same set have different values on this characteristic.) (2) Does this characteristic distinguish ethnic categories from the categories we classify as nonethnic? (3) Can this characteristic constitute the group or does it describe a group that is constituted on some other basis?

In each case, I experiment with several ways of interpreting each defining characteristic, choosing the interpretation that has the broadest coverage. Thus, in evaluating the characteristic of “a common region of origin,” I interpret the term “region,” what a “region of origin” is, and what having a region of origin in common requires in as expansive a way as possible. The purpose of doing this is less to discern the authors’ true intent than to ask: Is there any reasonable interpretation of this definition that captures our classification of ethnic identities?

Table 15.2 summarizes the results of this evaluation. It records whether or not the ethnic category in each row is distinguished from other ethnic categories in a comparable set by the characteristic listed in the column. As we see, most characteristics do not meet any of these criteria – and none meet all three. If no defining characteristic, taken singly, is both constitutive and distinguishes most of the categories that we call ethnic, it must also be the case that no combination of these defining characteristics can meet those criteria.

The remainder of this section gives examples of the reasoning which underlines the evaluation in Table 15.2. I refer the reader elsewhere for the details of the arguments (Chandra 2006, 2007).

Consider the characteristic of a common region of origin. At first glance, most of the categories in our sample appear to be associated with a common region of origin: Blacks in the United States with origin in Africa, Whites with origin in Europe or Eurasia or the Caucasus, Serbs with an area inland of the Dalmatian coast, Croats with northern Dalmatia, and so on. Indeed, of all the categories in our sample, only two are not associated with a common region of origin. Coloureds, descended from European migrants, Africans, and Indians in South Africa, do not appear to have a common region of origin. Nor do Hausa-Fulani, since they are constituted by an aggregation of two groups, each with a distinct point of origin: the Hausa, associated with an area of origin north of the confluence of the Niger and Benue rivers, and the Fulani, associated with a point of origin in lower Senegal.

TABLE 15.2. (Mis)match between Previous Definitions and Our Classification of Ethnic Identities

	Myth of			Myth of			Myth of			Myth of			Myth of		
	Common Ancestry	Common Ancestry	Common Region of Origin	Common Region of Origin*	Group Descent Rule†	Common Culture	Common Language	Common History	Common Territory	Common Territory	Conceptual Autonomy				
Black	No	No	No	Yes	No	No	No	No	No	No	No				
White	No	No	No	Yes	Yes/No	No	No	No	No	No	No				
Serb	No	No	No	Yes	Yes/No	No	Yes	No	No	No	No				
Croat	No	No	No	Yes	Yes/No	No	Yes	No	No	No	No				
Mohajir	No	No	No	Yes	No	No	Yes	No	No	No	No				
Punjabi	No	No	No	No	Yes/No	No	Yes	No	No	No	No				
Pathan	No	Yes	No	Yes	Yes/No	No	Yes	No	No	No	No				
Baluch	No	No	No	Yes	Yes/No	No	Yes	No	No	No	No				
Flemish	No	No	No	Yes	Yes/No	No	Yes	No	No	No	No				
Walloon	No	No	No	Yes	Yes/No	No	Yes	No	No	No	No				
Aymara	No	No	No	Yes	Yes/No	No	Yes	No	No	No	No				
Quechua	No	No	No	Yes	Yes/No	No	Yes	No	No	No	No				
Yoruba	No	Yes	No	Yes	Yes/No	No	Yes	No	No	No	No				
Ibo	No	No	No	Yes	Yes/No	No	Yes	No	No	No	No				
Hausa-Fulani	No	No	No	No	No	No	Yes	No	No	No	No				
Zulu	No	Yes	No	Yes	Yes/No	No	Yes	No	No	No	No				
Xhosa	No	Yes	No	Yes	Yes/No	No	Yes	No	No	No	No				
Coloured	No	No	No	No	No	No	No	No	No	No	No				

* This characteristic distinguishes the groups we classify as ethnic from groups we classify as nonethnic.

† This characteristic constitutes a group rather than presupposing it.

But there is no logical definition of a region that distinguishes categories in the same set from each other. Indeed, the identification of a common region in each case is a consequence of having identified the category first by some other means and then reasoning backward until we also find a common region of origin. We could just as easily associate categories in a comparable set with the same region of origin.

To illustrate, consider the case of Blacks and Whites in the United States. Some definitions of region and choices of time period would reveal distinct common homelands for both groups, while others would reveal a shared one. If we categorize Africa and Eurasia as distinct regions, then we can associate Blacks and Whites with distinct regions of origin and thus consider them distinct ethnic groups. But there is no analytical reason why we should think of Africa and Eurasia as distinct regions instead of disaggregating further within both categories. If we thought of regions in Africa, including present-day Liberia, Ghana, Cote d'Ivoire, Togo, Benin, and Cameroon, as distinct, then we would have not one but several ethnic groups corresponding to origin in these regions. Similarly, if we thought of regions in Eurasia, including France, Ireland, Germany, Lithuania, Poland, and Italy, as distinct, then we would also have several ethnic groups. The identification of Africa and Eurasia as regions of origin requires us first to define Blacks and Whites by some criteria other than region and then employ the definition of region of origin that captures present-day members of these predefined groups and excludes nonmembers.

Further, even if we take Africa and Eurasia to be distinct regions, arguing that Blacks and Whites originated in these two regions makes sense only when based on an arbitrary choice of time period. We can identify distinct points of origin for Blacks, for instance, if we start with the beginning of the slave trade in the sixteenth century. But if we continued going backward from this time, we would find a point at which both Blacks and Whites originated in present-day Africa (Cavalli Sforza 2000). And if we went forward, we could just as easily say that both groups share a common homeland – the United States.

The point applies to all the other groups in our sample and well beyond our sample. Indeed, we can even find a common point of origin for the ancestors of categories not commonly associated with one. Consider Coloureds in South Africa, whose ancestors are associated with Europe, Africa, and India – on the face of it, different regions of origin. We know that populations who lived in Europe, Africa, and India at later times in history migrated from a central core at an earlier time and that their descendants in this case migrated to South Africa. Thus, we could also argue that the ancestors of the Coloureds had a common point in origin, either by counting the early ancestors who migrated to Europe, Africa, and India or by counting the most recent ancestors, who lived in South Africa.

The strong association between the characteristic of a common region of origin and the categories that we call ethnic is not an indication of the power of this defining characteristic but its weakness. We can find this characteristic in any set of groups, ethnic or nonethnic, by tailoring our definition of region to

that group and/or tailoring the point in time to which we trace the ancestry of some present-day individuals who belong to that group.

The same reasoning applies to the defining characteristics of a common ancestry, a common territory, and a common history. It may also apply to a common language. Because I try to err on the side of inclusion rather than exclusion in my evaluation, Table 15.2 takes the existence of a common language at face value and describes a category as being distinguished by a common language if an entry in the *Encyclopedia Britannica* describes it as such. But when looked at closely, the existence of a common language is often a product of ethnic differentiation rather than an indicator of it. The association of separate languages – Serbian and Croatian – with Serbs and Croats, for instance, is a recent development that occurred after a long period of polarization. Zulu and Xhosa, similarly, although now treated as distinct languages with distinct conventions, are both part of the same language group and could in principle be seen as belonging to the same language. And although Quechuas and Aymaras are associated with distinct Quechuan and Aymaran language families, both could equivalently be described as belonging to the same Quechumaran language family.

Consider now the characteristic of a *myth* of a common region of origin. Many categories in our sample are indeed associated with this myth. And this defining characteristic does appear to distinguish ethnic from nonethnic categories. But this characteristic cannot be considered constitutive since its credibility is conditional upon the descent-based attributes that distinguish members from nonmembers.

To illustrate, imagine an attempt to propagate the myth that college professors in Nigeria are descended from those who once shared a region of origin. If all we knew about the individuals described by this myth is that they are college professors, this myth would be difficult to take at face value. But suppose we were also told that these college professors have a patrilineal descent rule, or the same last name, or the same mother tongue, or some other shared characteristic that justifies thinking of them as having ancestors who once inhabited the same region. Given these characteristics, we might be more willing to accept this myth because of the presence of these other characteristics. If this is the case, then these descent-based characteristics that determine the credibility of the myth must be the primary defining characteristics, not the myth itself. The same point applies to other myths taken to be defining characteristics, such as a myth of common ancestry.

Even as a secondary feature, furthermore, this defining characteristic excludes three important classes of categories: “hyphenated” categories, created explicitly as the result of an aggregation of smaller ones, “mixed-race” categories, and, for want of a better word, “nonmigrant” categories. The Hausa-Fulani in Nigeria are an example from our sample of the first. As a category transparently formed as an amalgam of two distinct categories, each with a distinct area of origin, this category is not associated with a myth of a common region of origin. Coloureds in South Africa are an example of the second, along with many mixed-race categories in many countries outside our sample, including mestizos in

Nicaragua, Bolivia, Venezuela, Uruguay, Peru, Panama, Honduras, and Mexico; "Pardos" in Brazil; Creoles in Mauritius; and mulattos in Uruguay and Panama, which we typically do not think of as having a common region of origin but routinely classify as ethnic categories. Indeed, these examples have all been chosen from the list of categories classified as ethnic across our data sets. Many of these categories describe the populations of countries in South and Central America. Thus, this definition would exclude large numbers of categories for at least an entire continent. As for nonmigrant categories, the list is large. It includes categories whose members are regionally concentrated but not distinguished by myths of origin. Punjabis in Pakistan are an example of the category, along with Bengalis or Tamils in India, as well as "titular" categories in many countries – Italians in Italy, Lithuanians in Lithuania, Poles in Poland. Although these categories, like any other, could probably be associated with a common region of origin, they are not categories around which such a myth has developed in practice. Nonmigrant categories without a myth of origin also include categories whose members are distinguished by descent from those professing different religious beliefs, not by descent from those who have different regions of origin. Consider, for instance, Catholics and Protestants in Northern Ireland. We think of the conflict in Northern Ireland as a prototypical example of ethnic conflict. The same is true of Muslims and Serbs in the former Yugoslavia, Hindus and Muslims in India, Muslims, Christians and Druze in Lebanon, Shias and Sunnis in Iraq, and so on.

Consider now the only alternative defining characteristic that is constitutive – the group descent rule. This definition does not distinguish ethnic identities from nonethnic groups such as aristocrats, landowning families, and so on, in which membership can also depend on the group membership of one's parents. But the more important critique here is that even when most expansively interpreted, it is too narrow.

It excludes the categories "Black" in the United States and "Coloured" in South Africa, examples of categories in which membership depends on the racial characteristics – but not the group membership – of one's parents. The membership rule in the category "Black" in the United States, for instance, does not take the form "You are considered Black if your parents were Black." Rather, it takes the form "You are considered Black if either of your parents or any of your ancestors had any African ancestry." Thus, you can be Black if all of your ancestors but one were coded as White. By the same logic, the group descent rule would exclude other race-based categories such as "Creole" or "Mestizo" in parts of South and Central America and "Coloureds" in South Africa for most of their history.

It also excludes categories such as "Hausa-Fulani," created as the result of the aggregation of smaller categories, or the disaggregation of larger categories into smaller ones. An individual might be considered Hausa-Fulani not only if both of her parents are Hausa-Fulani, but also if her parents are either Hausa or Fulani. Such a category would not be classified as ethnic by this membership rule.

Indeed, this is the reason that this definition excludes the early generations of many categories in our sample. Many categories are initially created as an aggregation or disaggregation of smaller categories. Consider the category “Yoruba” when it was invented in Nigeria in the nineteenth century. At this time, the parents of those who were classified as Yoruba were not themselves classified as Yoruba because this category did not exist during their lifetimes. According to the group descent rule, then, the category “Yoruba” in the nineteenth century would not be coded as ethnic. But the category “Yoruba” is universally coded as an ethnic category by all comparative political scientists without making a distinction between time periods.

Similarly, take the example of the category “Serb,” believed to be mentioned for the first time in the sixth century A.D. Those who came to be categorized as Serb at some point after the sixth century A.D. had parents who were not known as Serb, but were members of one or more South Slav tribes who had settled in Southern Europe and had other characteristics, such as an association with the Orthodox Church and exposure to the Cyrillic alphabet. It seems reasonable to assume, based on what we know of the history of the region, furthermore, that the membership of the category “Serb” was not decided once and for all at this time; it must have expanded during the Middle Ages, to cover new migrants to the region or new subjects of the Serb kingdom or new converts to the Orthodox Church or new speakers of the local dialects, and also contracted to exclude those who migrated out of the region, kingdom, church, or dialects. But according to the group descent rule, the early generations of all those who came to be called Serb would not be counted as having a Serbian ethnic identity because their parents were not called Serbs.

Finally, the group descent rule excludes categories created as the result of migration or territorial change. The identity category “Mohajir” is an example. This category describes those who migrated to Pakistan from North India and their descendants. The parents of many of those classified as Mohajir were not classified as Mohajir themselves, since the category did not exist while they were alive – and since they themselves were not migrants, as their children were. Applying the group descent rule would lead us to code the first generation of Mohajirs as a nonethnic group to the extent that membership in this category did not depend upon being able to code one’s parents as Mohajirs. The same would apply to other migrant groups – Irish Americans in the United States, or the Chinese in Malaysia, or the Nepalese in Bhutan, for example. Yet, all of our data sets count the Mohajirs as an ethnic group independent of time and generation, as they do these other migrant groups.

Consider, finally, a common culture as a defining characteristic. This, as we have seen, is a recurrent feature in several definitions. But no matter how we define culture it cannot be taken to be a defining feature of ethnic groups – at least ethnic groups as we classify them. Many of the groups we classify as ethnic do not have a common culture; many “different” ethnic groups do not differ based on culture; and many of the groups distinguished by a common culture are not classified as ethnic. Consider a standard definition of a common culture as a shared set of

symbols, values, codes, and norms (consistent with Rogowski, cited in Wedeen 2002). If we require group members to share all values and codes, that would probably disqualify almost all the groups that we count as ethnic. Take the category “Yoruba.” Those classified as Yoruba share some aspects of culture so defined – for instance, a common myth of origin, the worship of a common set of deities, and a common language. But there are also many aspects of culture that they do not share. Yorubas who trace their origin to different ancestral cities in Yorubaland, for instance, speak different dialects of the Yoruba language, have localized festivals, and have distinct myths of origin, institutions, and rituals. Christian and Muslim Yorubas not only have different modes of religious practice and observance but different sets of value orientations (Laitin 1986). The symbols, codes, and norms of those classified as Black similarly differ a great deal: There are actual or perceived differences between Jamaican and American Blacks based on language, work ethic, and family values (Waters 1999), between Blacks from the North and the South (Lemann 1992), between middle-class and poor Blacks, and between urban and rural Blacks (Malcolm X 1964). Without belaboring the point, we could describe similar heterogeneity in symbols, values, and codes within each of the ethnic categories in our sample. At the same time, this definition would include many categories that we do not consider to be ethnic – professors, for example, and other occupational groups – which do have a common culture defined in this way, with common symbols, values, and codes, defined and maintained by institutions set up explicitly for this purpose.

PROPERTIES OF ETHNIC IDENTITY CATEGORIES

Here I identify two properties of ethnic categories – constrained change and visibility – that can be associated with ethnic identities by definition – and list several properties that cannot, either because they are based on definitions that I eliminated earlier or because they contradict the properties derived from this definition or because they are not based on my definition at all.

The properties of constrained change and visibility are derived from the properties of descent-based attributes for membership. Descent-based attributes, by their nature, are either displayed on the body or must be consistent with the attributes displayed on the body. This association with the body gives these attributes two intrinsic properties: stickiness and visibility. The stickiness and visibility of descent-based *attributes*, in turn, produce the properties of constrained change and visibility in ethnic identity *categories*.

Constrained Change

Attributes associated with, or believed to be associated with, descent are, on average, difficult to change in the short term. In contrast, attributes not associated, or not believed to be associated, with descent can, on average, be easily changed even in the short term. This claim, illustrated in Figure 15.1, refers to the average tendency of *distributions* of attributes rather than any single one.

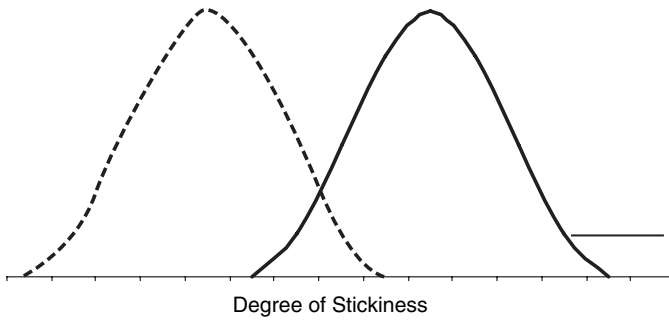


FIGURE 15.1

Imagine a scale that orders all attributes according to the degree of difficulty associated with changing them in the short term. Descent-based attributes are distributed in the upper half of this scale, while others are distributed in the lower half of this scale, with an area of overlap in between.

To illustrate, let's go back to Helen. One of the attributes given to her at birth – skin color – lies at one extreme on the scale of stickiness. Skin color can, with medical help, be changed within a single generation, and there are a handful of cases where it has been (Griffin 1996; Larsen 1997). Skin color can also be changed through a change in the interpretive framework within which it is viewed: If a sufficiently large number of external observers adjust their frameworks, Helen's particular shade of skin color might be reinterpreted as light rather than dark. But such a change is likely to be made with great difficulty and over a period of time.

There are degrees of difficulty. Changes in skin color and gender are likely to be most difficult. Changes in physical features are likely to lie in the middle, in part because of the constraint of making a change in any one feature appear consistent with others. Helen might easily change her hair type, for instance, by having it curled or her eye color by wearing colored contact lenses. But in order to make the myth that these new attributes were given at birth credible, she would also need to change other attributes, such as her skin color and facial features, to make them consistent with that myth. Changing the place of birth of one's parents and ancestors, or one's last name, or one's ancestral religion may be least difficult, especially in the context of relocation or migration, when new histories can be more easily invented.

In contrast, attributes not based on descent, or not believed to be based on descent, are, on average, easier to change. Helen might change her educational qualifications, for instance, simply by returning to school. She might alter her income by asking for a raise – or both her occupation and income by getting a higher-paying job. She can bring about these changes without having to erase her previous attributes. While she cannot claim birth in the United States without renouncing her birth in Trinidad, getting a master's degree does not require her to disguise the fact that she has a high school diploma.

Here as well, there are degrees of difficulty. Changing your initial endowment of economic resources – acquiring more land, for instance – can in some contexts be almost as difficult as changing your skin color. Changing your educational credentials is often less difficult – but it requires a significant investment in time and money. And changing jobs or party membership is perhaps easier. But the degree of difficulty here depends on the context rather than on some intrinsic property of such attributes.

There is also an area of overlap in the degree of stickiness of individual attributes that fall into the two types. Some attributes that are not descent-based – such as land ownership or accent – may lie toward the high end of the scale of stickiness. Other descent-based attributes, such as one’s last name or the religion of one’s parents, may lie toward the low end to the extent that the myths required by these changes may be easily invented. Such overlap is consistent with differences in the average tendency of both types of attributes.

It is common to assume that because the *attributes* defining them are fixed in the short term, ethnic identity *categories* are also fixed in the short term. Individuals can change their identity categories, often quite rapidly, by combining and recombining elements from their set of attributes differently. But the pattern of change should be constrained by the underlying distribution of attributes. Thus, the property of constrained change can be legitimately associated with ethnic identities in the short term, while the property of fixedness cannot.

Consider Helen again. Helen’s stickier attributes consist of her skin color, her birth in Trinidad, and her descent from parents of African origin. While she cannot change these attributes in the short term, she can, by selecting different attributes, change the categories in which she activates her membership. If she emphasizes the attribute of birth on the English-speaking island of Trinidad, she can activate membership in the category “West Indian.” But if she deemphasizes her foreign birth and highlights instead the attributes of skin color and descent, she can activate membership in the category “Black.” Her ability to change the categories that she activates is constrained: While she can change to categories defined by a different selection of sticky attributes that she possesses, she cannot change to categories based on sticky attributes that she does not possess (e.g., German, if that category is defined on the basis of birth).

Visibility

A second property of descent-based attributes is that they are, on average, more visible than attributes acquired in the course of a lifetime. By “visible attributes,” I mean attributes that can be ascertained through superficial data sources such as the name, speech, features, and dress of individuals. The larger the number of sources that contain information about an attribute, the more visible it is. By “invisible attributes,” I mean attributes that can be ascertained only by more careful observation and background research (Chandra 2004). As noted earlier, this claim refers to distributions rather than particular attributes.

Once again, let's go back to Helen to illustrate. Some of Helen's descent-based attributes, such as her gender, skin color, hair type, and physical features, are immediately displayed on her person. Others, such as the language of her parents (English), are contained in her name. Less precise information on the religion of her parents is also available in her name, which, even though it does not allow observers to pinpoint it precisely, does allow them to rule out religions such as Islam or Hinduism. The way in which observers code such information is the result of social construction. Helen's shade of skin color, for instance, might be interpreted in the United States as black but in Brazil as brown. This difference is the result of differences in historical and institutional contexts that have taught individuals to instinctively code the same data in different ways (see Chandra 2004). But the main point is that some raw data about some of the attributes given at birth are always visible, often in multiple data sources, regardless of how the observer interprets them.

Information about attributes not associated with descent can also be visible. Perhaps the most visible non-descent-based attribute is age, always visible in speech and physical features. Education, income, and place of residence (rural or urban) are sometimes visible in accent and appearance, but typically not in the person's name or features (unless they are correlated with descent). Profession or organizational membership can be visible in dress (the suits that lawyers and bankers wear, the uniforms of policemen and firemen, badges and rosettes signaling political party membership, etc.) but not when an individual is "off-duty" and also not in the name or features. But on average, attributes not based on birth are less visible and less frequently visible.

For instance, Helen's occupation as a food-service worker is signaled during working hours by her uniform. And when she is wearing her uniform or when she is at work, her income and educational background may be inferred from her occupation. But when she is dressed differently and/or off duty, her nonbirth given attributes immediately become less visible. In contrast, some information about her descent-based attributes is always visible, no matter what the context of observation, in her name and features.

Here, as earlier, there is an overlap in the degree of visibility associated with both types of attributes, probably larger than the area of overlap associated with the stickiness of both types of attributes. Gang membership is not given at birth, but, when proclaimed in tattoos, haircuts, and other marks on the body, it can sometimes be more visible, and more frequently visible, than many descent-based attributes. And the religion of one's parents, while given at birth, can often be less visible than one's educational qualifications or age.

The link between the property of attributes and categories is straightforward in this case. When attributes are visible, membership in the categories based on them will also be visible. But note that visibility can coexist comfortably with multiple identities and with error (Chandra 2004). The information displayed on the person can make several identity categories visible, not just a single one. And even though information on an individual's declared and potential ethnic memberships is visible, observers can often get it wrong. Observers might well

miscode Helen's category membership as "Nigerian" or Brazilian," for instance, even when the correct guesses are "West Indian" or "Black." But the main point is that the attributes that define membership in our ethnic categories are visible enough, on average, to permit such guesses.

Note, further, that the properties of constrained change and visibility apply to all descent-based identities rather than to ethnic identities specifically – and apply to all descent-based identities only on average. Since ethnic categories are only a subset of categories based on descent-based attributes, we should also see constrained change and visibility in some categories that are based on descent-based attributes but that we do not think of as ethnic – for example, gender. But because some attributes associated with descent are either not sticky or not visible, ethnic categories based on such attributes may not exhibit the properties of constrained change and/or visibility. And finally, since some non-descent-based attributes may also be sticky and/or visible, we might also see the properties of constrained change and visibility associated with some categories that are neither ethnic nor descent-based.

The two properties identified here need not be exhaustive; there may well be others that apply precisely and uniquely to ethnic identities as identified here. One promising direction in which to proceed in identifying others is to explore the fact that offspring of the same parents typically share the same set of ethnic identity options. It may well be that because individuals are more emotionally connected to siblings than nonsiblings, membership in ethnic categories should arouse greater emotional attachments, or create a greater tendency to demonize ethnic others, or increase the stakes of conflict, than membership in other types of categories, even those based on descent (e.g., gender). By the same logic, if we assume that individuals care more about the well-being of siblings than more distant connections, we may see a greater degree of within-group altruism among members of ethnic categories than among members of other types of categories.⁷ This may be a critical difference between ethnic identity categories such as "Black," other descent-based identities such as "women," and non-descent-based identities such as "Republicans." Even when these other identities are also visible (e.g., women) and sticky (e.g., Republican in the United States), they often cut across siblings, and this may be a consequential fact. Such properties are routinely associated with ethnic groups by comparative political scientists but without justification. The definition proposed here provides an analytical foundation on which to infer such an association or establish its absence.

We can also now identify three classes of properties that are *not* intrinsic properties of ethnic identities.

The first such class of properties includes those associated with ethnic identities by definitions that do not match our classifications. These include the 10 properties identified in eliminated definitions such as (1) a common ancestry, (2) a myth of common ancestry, (3) a common place of origin, (4) a myth of a

⁷ I was introduced to this idea by Will Le Blanc, a doctoral candidate in the Department of Political Science at MIT.

common place of origin, (5) a group descent rule for membership, (6) a common culture, (7) a common language, (8) a common history, (9) a common territory, and (10) conceptual autonomy. All of these properties, I have argued, either do not distinguish the many identity categories that we routinely classify as ethnic from each other or from nonethnic identities or describe an identity category defined by other means. While we may sometimes find ethnic identities characterized by these properties, they are best thought of as variables rather than constants – and variables that themselves require explanation before they can be incorporated in theories of ethnicity and other outcomes.

The second such class includes the properties that contradict the definition that does capture this classification: fixedness and exogeneity. If ethnic identities change within constraints, then they cannot by definition be fixed. And if such change occurs as a product of human action, then they cannot by definition be exogenous to that action.

The third such class includes those properties that are not associated with ethnic identities in any definition or by a cursory look at the data.

Take, for instance, the property of “networks.” This property is not implied by any of the definitions I have discussed. And even if we find that members of ethnic groups, however defined, do share network ties, we know, by observation, by anecdote, and by a wealth of literature on this subject that so do those who go to the same school, belong to the same profession, live in the same neighborhood, have the same hobbies and so on. Networks, then, cannot be taken as intrinsic properties of ethnic identity, at least on the basis of what we know so far.

The same point applies to properties such as “institutional ties” or “norms.” Institutional ties, for instance, characterize some groups that we think of as ethnic (e.g., Shias in Iraq, linked by membership to mosques) but also groups that we think of as nonethnic (e.g., trade unions that link industrial labor or political party organizations that bring together the party faithful). They also do not characterize some groups that we think of as ethnic (e.g., Hindus in India have not, for most of their history, been linked through institutional affiliations) and some groups that we do not think of as ethnic (e.g., peasants, who historically have had a worse record of organization than the working class or the urban poor).

THE WEAK PRESUMPTION THAT ETHNICITY MATTERS IN OUR CAUSAL CLAIMS

Only a handful of our causal claims rest on the intrinsic properties of ethnic identity. Among these claims are Caselli and Coleman and Fearon’s recent work on patronage and ethnic mobilization (Fearon 1999; Caselli and Coleman 2001), Janet Tai Landa’s work on trading networks (Landa 1994), and my own work on patronage (Chandra 2004). Although none of these claims were made on the basis of a consistent definition, they are robust to evaluation against the one proposed here. Caselli and Coleman and Fearon argue that there should be an association between patronage politics and ethnic politics because patronage

politics favors coalitions based on identities that are hard to change, and ethnic identities are hard to change. Ethnic identities, I suggest, are not quite as hard to change as Caselli and Coleman and Fearon argue; it can be easy for individuals to switch between ethnic identities within the constraint of the underlying set of attributes. But to the extent that change in ethnic identities is more constrained than change in nonethnic identities, the argument is upheld. Landa (1994) argues that under conditions of uncertainty, the visibility of ethnic cues allows individuals to select trading partners and enforce contracts. Chandra (2004) argues that given the information-constrained environment of elections in a patronage democracy, the visibility of ethnic identities makes them more likely to be activated in voter and party behavior than nonethnic identities. While these arguments remain to be tested empirically, their reliance on properties that can be considered intrinsic to ethnic identities at least makes them logically sustainable.

However, the arguments made even in these few works should be read as applying to distributions of ethnic and nonethnic identities rather than to each type of identity individually. If we consider individual identity types, we will certainly find some identities that are ethnic and that do not have constraints on change or are not visible and thus do not exhibit any particular affinity with patronage. And we will certainly find some identities that are nonethnic but either have constraints on change or are visible or both – and thus should exhibit an affinity with patronage.

By far the largest number of explanatory claims about ethnicity rest on properties that I have argued are *not* intrinsic to ethnic identities. These include the family of arguments linking ethnic identities with various forms of violence, which rest on claims such as fixedness, a common history, dense social networks, and distinct emotional responses. They also include a subset of the arguments linking ethnicity to patronage, which rely on the properties of networks, institutional ties, and spatial concentration. And they include arguments linking ethnic identity with the destabilization of states and regimes, which employ properties such as territorial concentration, a division of labor and fixedness.

As such, they cannot be taken as reasonable claims about the effect of ethnic identities in general. They should be reformulated as claims about a specific subset of ethnic identities, or claims about the effect of ethnic identities combined with some additional variable. Rather than reading them to mean that "Ethnicity is associated with some dependent variable Y," we should read them to mean either that "Some set of identities, which sometimes includes ethnic identities, is associated with dependent variable Y" or that "Ethnicity, along with some other variable X, is associated with the dependent variable Y."

As an example of a causal claim that refers to only a subset of identities, consider one that explains the association between modernization and ethnic politics by invoking the property of territorial concentration (Bates 1974: 464). The argument is informed by the study of ethnic politics in Africa, and it

certainly appears to be the case that several ethnic categories in Africa are territorially concentrated. But we cannot take either territorial concentration or the memory of a common territorial homeland to be a defining property. Nor does the argument describe ethnic categories in general. And we can certainly imagine nonethnic categories that are territorially concentrated. This argument thus should be read not as a general argument about ethnic identities but as an argument about territorially concentrated identities, a concept that may or may not overlap with ethnic identities.

As an example of a causal claim that probably omits some variable, consider the argument that democracies are destabilized by the permanent exclusion of some minority groups from power. The key property of ethnic identity that this argument rests on is fixedness: If ethnic identities are fluid, not fixed, then the other propositions fall through. But we know, based on the definition proposed in this chapter, that fixedness is not an intrinsic property of ethnic identities – constrained change is. If we find that ethnic identities consistently acquire fixedness in a democratic context and that fixedness, in turn, threatens democratic stability, it must be due to some extrinsic variable that interacts with ethnic identity that has not been theorized.

HOW TO CONSTRUCT CAUSAL CLAIMS WITH STRONGER PRIORS

The best way to address the weakness of our presumptions in many cases may be to abandon them and to start to theorize about identities in general, or about new classes of identities that may include some identities we call ethnic but are not defined by ethnicity: institutionalized identities, majoritarian identities, cultural identities, and so on. But to the extent that there are at least two (and perhaps more) properties disproportionately associated with ethnic identity, there may indeed be some causal claims about the distinct effect of ethnic identities that are reasonable. The implication of the argument made here, then, is not that we should do away with the presumption that ethnic identity, or concepts related to ethnic identity, may indeed have some distinct causal effects on political and economic outcomes, but that there are specific guidelines we should follow in making and interpreting claims based on this presumption.

Disaggregating Ethnicity

Theories of the relationship between ethnicity, politics, and economics typically employ a handful of concepts, each big and blunt, to analyze the effect of ethnicity. According to the definition of ethnic identity proposed here, however, ethnicity is not one big concept, or three, but many tens of narrow ones. Causal claims about ethnicity, then, should trace not a handful of blunt causal paths from ethnicity to the outcome of interest, corresponding to a handful of blunt concepts, but many tens of precise ones.

If we want to learn how ethnicity is related to patronage, for instance, we should ask at least the following questions: Does the distribution of attributes in a

population affect patronage? What about the degree of stickiness of these dimensions? What about the relationship of these dimensions to each other? What about the size of nominal identity repertoires? What about the degree of overlap of attributes between categories in this repertoire? What about the categories activated at the local level in politics? Categories activated at the national level? Network ties across activated categories? The rate of change between activated categories? The degree of inclusion of activated categories in government? And so on.

Different concepts related to ethnic identity may have different relationships with the outcome of interest. It may well be, for instance, that concepts related to ethnic structure have no relation with democratic stability, while concepts related to ethnic practice might. And it may well be that concepts related to ethnic practice have different causal relationships: For example, the implicit activation of ethnic identities in political campaigns may well have a different result from the explicit activation of ethnic identities in these campaigns.

What is more, these paths may very well connect, in logical and dynamic relationships, since the concepts to which they are related connect, in logical and dynamic relationships. For example, the effect of the distribution of attribute repertoires on patronage may well be related to the effect of the size of activated ethnic categories on democratic stability, since the second is a product of the first. And, because a change in activated categories can lead over time to a change in the underlying attribute dimensions, we should expect there also to be a dynamic relationship between these two concepts and democratic stability over time. Employing the conceptual framework offered here, then, leads us to look for, and enables us to model, interdependent causal effects.

Making Claims Based on Intrinsic Properties

Second, to the extent that ethnic identities are only distinguished by their reliance on descent-based attributes, the only intrinsic properties that can reasonably be associated with them in our theories should be those derived from the properties of these attributes. Here I have highlighted two such intrinsic properties of ethnic identity categories – constrained change and visibility – derived from two properties of descent-based attributes, although there may well be others that remain to be identified.

At the same time that this imposes a restriction on the range of claims that we can make, it also opens up new areas of inquiry by introducing a new perspective to previous theories about the relationship between ethnicity, politics, and economics. Consider the relationship between some concept related to ethnicity and democratic stability. Many of our theories about this relationship rest on the assumptions of fixedness and exogeneity. Given that these properties cannot reasonably be associated with ethnic identity, we have no reason to believe that these theories, as they are specified, are reasonable. But perhaps there is a relationship between the properties we can reasonably associate with ethnic identity and democratic stability. Might ethnic identities produce distinct patterns of

politics because they are more visible on average – and therefore distinct average effects on democratic systems? Might ethnic identities have a distinct effect because they change within constraints? Further, there may be the possibility that variables that describe all sorts of identities can produce effects unique to ethnic identity by interacting with properties that apply uniquely to ethnic identity.

Restricting our claims to properties that can be taken to be intrinsic to ethnic identities also leads us to ask entirely new questions. These include: Does the possibility of change in ethnic identity make violence less likely? Why do some identities *not* change? Do different types of identities (racial, religious, caste, clan, and so on) have distinct degrees of visibility or stickiness? Do particular types of constraints on change have different effects? Might there be a relationship between the degree of visibility of an identity and the degree of constraint on change?

Indeed, even the elimination of definitions proposed here leads us to ask new questions. For instance, I have argued that a common region of origin does not distinguish ethnic identities objectively speaking. But a *belief* in an ancestral region may well affect behavior even when it is not based in fact. Similarly, while the properties of fixedness and exogeneity are not constitutive of ethnic identities from an analytical perspective, members of activated ethnic categories may indeed see these categories as fixed and exogenous. Might this belief be associated with distinct effects (Suny 1999; Gil-White 2001)? We can begin to ask such questions only when we stop thinking of these characteristics as definitions and begin thinking of them as beliefs, often variable, associated with ethnic identities defined by some other means.

Making Claims about Distributions, Not Dichotomies

Finally, to the extent that the intrinsic properties that distinguish ethnic identities from nonethnic identities distinguish the average tendencies of distributions of both identities over time and space rather than each identity in each type individually, at any particular point in space and time, our causal claims about the effects of ethnic versus nonethnic identities should be interpreted and evaluated as claims about the average trends associated with *distributions* of each type of identity over space and time and not as claims about relationships between individual types of identity at any single point in space and time.

In order to make more precise claims that apply to a dichotomous classification of identities rather than to distributions, and that can be used to generate point predictions rather than trend predictions, we should replace the concept of ethnic identities with the concepts of sticky and visible identities. Any claims about ethnic identities and descent-based identities are necessarily imprecise because they rely on the underlying properties of stickiness and visibility, which are disproportionately related to these identities. But if we theorize about the effects of stickiness and visibility themselves, then we should generate more precise results.

Ultimately, if we are unable to identify any further properties that are unique to ethnic identity, we would be better off substituting the concept of ethnic

identity in our theories with concepts such as descent-based identities or identities based on sticky or visible attributes. But if we do, does this mean that this attempt at definition was wasted? To the contrary. The negative claim, that ethnicity does not matter, is a discovery of great magnitude. It should have far-reaching consequences for research and data collection, suggesting that we should abandon the large number of theories and data sets that we have on ethnicity and start again on an entirely different foundation. A claim of this magnitude cannot be made lightly. It must be justified perhaps even more strongly than the claim that ethnicity matters on a conceptual basis. And, just as we need to define the concept of ethnic identity to establish that it matters, we also need a definition to establish that it does *not* matter and why. We also need a definition that tells us what to replace the concept with – and what not to. Otherwise, we will go round in circles, creating new terms that repeat the errors of previous work.

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